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The A Priori
in Physical Theory

The A Priori
in Physical Theory

ARTHUR PAP

NEW YORK / RUSSELL & RUSSELL

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ACKNOWLEDGMENT

I am indebted to Professor Ernest Nagel for many clarifying instructions and discussions. I hardly exaggerate in confessing that I owe my conversion from the metaphysical "pathos of obscurity," bred by careless and pompous ways of using language, to conscientious endeavors of clear, rigorous thinking mainly to his philosophical influence on my mind. The portions of the present work that owe their final shape to his unrelenting criticism are too numerous to admit of detailed description.

My discussion of Kant's "transcendental analytic" has been inspired by late Professor Ernst Cassirer's illuminating interpretations of Kant. The writing of the present work was begun, in 1944, at Yale University, under the friendly and encouraging guidance of the late German scholar. If this were the place for autobiographical details, I would express the great pride I take in having been privileged to be a focus of so much serenity and sweetness which Ernst Cassirer radiated in personal intercourse.

To Professor J. H. Randall Jr. I am indebted for helpful suggestions concerning revisions of the original version of the present work.

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FOREWORD

Les principes sont des conventions et des définitions déguisées. Ils sont cependant tirées de lois expérimentales, ces lois ont été pour ainsi dire érigées en principes auxquels notre esprit attribue une valeur absolue (Poincaré, La Science et l'Hypothèse)

THIS QUOTATION from Poincaré indicates the underlying idea of this essay. Under the influence, first, of C. I. Lewis' "conceptual pragmatism," as developed in *Mind and the World Order*, and then of Dewey's *Theory of Inquiry*, I was led to develop a functional interpretation of the *a priori* with close regard to the methods of physics. My interest in the exact sciences, awakened by Prof. Ernest Nagel, and the stimulus I received from the late Prof. Ernst Cassirer's endeavors to make Kant intelligible in terms of physics, are jointly responsible for the attempt, set forth here, to substantiate "conceptual pragmatism" in terms of the procedures of physics. The dictum that in so far as a statement is *a priori* it is verbal and "asserts nothing about reality" and in so far as it is synthetic it may be refuted at any moment by experience, always left me with a sense of mental discomfort. After several attempts at rehabilitating the honorable status of "synthetic *a priori*" propositions had failed, the conventionalist writings of Duhem and Poincaré, and especially Victor Lenzen's *The Nature of Physical Theory*, helped me to locate the trouble. If, as methodologists, we adopt a static point of view, and examine the body of scientific propositions as it may be found systematized at a definite stage of inquiry, we will, indeed, successfully divide it into analytic and synthetic propositions, as forming mutually exclusive classes. If, however, our point of view is dynamic or developmental, we shall find that what were experimental laws at one stage come to function, in virtue of extensive confirmation by experience, as analytical rules or "conventions," in Poincaré's language, at a later stage. This fact is, for example, illustrated in the "embodiment," so to speak, of laws in measuring instruments: in the context of experimental inquiry, in which a certain measuring instrument is employed, the law in accordance with which the indi-

cations of that measuring instrument are interpreted, is no doubt *a priori* in the sense of being irrefutable by the results of the experimental investigations. Still, no physicist would regard such a law as an arbitrary "rule of procedure" and deny to it its empirical foundation and contingency. The only way to solve this apparent paradox is to recognize the development of what are *results* of experience at one stage into "constitutive *conditions*" of experience, to speak Kantian language, at another stage.

Methodologists have a tendency neatly to compartmentalize scientific propositions into, say, theoretical, experimental, procedural. But through this tendency of categorization they are prone to miss the flexibility of scientific inquiry, and hypostatize functional distinctions into distinctions of inherent type. Thus it would no doubt be extremely difficult to find, in any branch of physics, a set of fixed procedural rules by which the coordination of theory and experimental data is effected. Actually, theories themselves are used to interpret experimental data, and to endow them with evidential relevance with respect to other theories; this being one sense in which physical theories may be said to be *functionally a priori*. The immediate experimental data that have to be interpreted in order to constitute relevant confirmatory or disconfirmatory evidence for a given theory, are pointer coincidences. The rules by which this interpretation is effected are the theories in accordance with which the measuring instruments have been constructed. Thus, if Boyle's law is to be tested, the indications of a thermometer have to be interpreted: the rule of interpretation, here, is the law of thermal expansion, in virtue of which the length of a thermometric liquid may be used as a measure of temperature. In analogous ways, the pointer coincidences of spring balances are interpreted by Hooke's law, those of platform balances by Archimedes' law of the lever, etc. In fact, every law expressed in equational form is a potential means of measurement, since a function is a measure of its argument, provided it is single-valued and monotonic.

The theory of the *a priori* which will, in this essay, be presented and applied to physical principles, may be called *functional* in so far as the *a priori* is characterized in terms of functions which propositions may perform in existential inquiry, no matter whether they be, on formal grounds, classified as analytic or as synthetic. It may also be called *contextual*; for statements of the form "x is *a priori*" or "x is *a posteriori*" (where the admissible values of x are propositions) will be treated as elliptical or incomplete. A proposition which is *a priori* in one context of inquiry, may be *a posteriori* in another context. Following

Victor Lenzen, I have laid special emphasis on the transformation of inductive generalisations into conventions, or the empirical origin of scientific definitions

C. I. Lewis, in *Mind and the World Order*, characterizes a priori propositions as "criteria of reality." A proposition of the form "all X are Y" is a priori, according to Lewis' usage of the term, if a phenomenon that does not exhibit the property Y would not be *categorized* as an X, or would not be subsumed under the concept X

. . . There is only one ground on which the necessary connection of X and Y, such that all X's will certainly be Y's, can be known; that is, that if we find that the concept Y is inapplicable to any particular, then the concept X will be retracted as likewise inapplicable. If we know with certainty in advance that all men are mortal, we know it because if we discover any being not to be subject to the accident of death, then, however like a man he may appear, we shall refuse to recognize him as human.¹

" . . . But the proposition 'all swans are white' is an empirical generalisation because white color is not included as essential in the denotation assigned to 'swan'."² And L. S. Stebbing, who advocates Wittgenstein's linguistic theory of the *a priori*, says. ". . . when we say that something *must* be so and so, we are saying something about the way in which we use, and intend to use, words On this view a priori propositions are, in a certain respect, analogous to statements of definitions; they are explicative."

. . . When we believe that X *must* be Y, we are never disappointed, for that which is not Y we refuse to consider X, however much it may have appeared to be X before we noticed it was not Y On this view, the assertion of *must* is clearly independent of what happens to be the case, and is thus neither derived from experience nor capable of being established by reference to experience³

Both Lewis and Stebbing, the latter endorsing Lewis' theory of the *a priori* except for substituting for Lewis' legislative function of *categories* the legislative function of *words*, pay little attention to the *development* of necessary "explicative" propositions (or, rather, the definitions that are the source of "dialectical" necessity) from empirical generalisations. Lewis, in the above quotation, contrasts the *essential* connection of humanity and mortality with the *accidental* connection of being a swan and being white. Yet, whether a property is essential or accidental is altogether relative to factual knowledge and

the objectives in terms of which one classification or definition is preferable to another. If in the progress of biological investigations there should emerge a causal connection between the defining characteristics of swans and their white color, biologists will tend to "retract" the concept "swan" as "inapplicable" to a non-white bird (instead of constructing a subspecies of "black swans"); the proposition "all swans are white" would then, by Lewis' *pragmatic* criterion, be *a priori*. Consequently, Stebbing's contention that "the assertion of *must* is clearly independent of what happens to be the case" is acceptable only if we envisage a temporal cross-section of inquiry, as it were, in which the definitions from which necessity derives are "ready made," and ignore the *empirical origin* of those very definitions.

Concerning the transformation of inductive generalisations into definitions, two preliminary remarks are in order

1) The spirit of the present account is radically alien to a Leibnizian rationalism, according to which it is but the weakness and finitude of human knowledge which hides from our intellectual sight the necessary character of those truths that appear to us to be merely contingent and factual. If analyticity is said to be an ideal limit to which inductive generalisations tend to converge, it is not implied that with the progress of knowledge all factual truths will be revealed as necessary "truths of reason." For *a priori* and *a posteriori*, *necessary* and *contingent*, are polar or correlative categories. Laws function analytically in an inquiry whose objective is the acquisition of factual knowledge; to make all laws *a priori*, to conventionalize all truth, would hence be defeating the very purpose which the *a priori* serves: it would amount to the contrivance of tools that have no use.

2) If the present analysis emphasizes the conventionalization of inductive truths, it is not thereby implied that this process may not sometimes be reversed. Once a conditional statement has become analytic, it is, indeed, exempt from *direct* empirical control. But experience, having suggested the law "all S are P" and thus mediated the definition of 'S' in terms of 'P,' may, through further investigation, reveal that S, as defined thus, is an inapplicable concept. In revising our concept of S, we implicitly acknowledge that P is not a truly invariant characteristic of S's, and thus experience indirectly controls our definitions. If 'S' is defined in terms of 'P,' it is impossible to find an S that is not a P. But what remains possible is that the predicate SP define an empty class; or that we find instances which exhibit all the properties of S except P, such a discovery suggesting a revision of our definition of S.

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The A Priori
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PART ONE

THE FUNCTIONAL A PRIORI

I. LEWIS' CONCEPTION OF THE A PRIORI

SINCE the following analysis is heavily indebted to C. I. Lewis' pragmatic theory of the a priori, it may be in order to open the discussion with a brief outline of that theory. In the rationalistic tradition two conceptions of the a priori prevailed: the Cartesian doctrine of necessary, self-evident truths, forming the immutable basis of whatever further truths may be derived by the process of ratiocination, and the Kantian doctrine of the a priori as legislative for the nature of experience in general. Lewis reveals the inadequacy of the former conception by pointing to the *hypothetico-deductive* character of scientific inquiry. The "first principles" of empirical science are not self-evident, but hypotheses that are validated in terms of empirical verification of their deductive consequences, and are open to abandonment or revision if their deductive consequences fail to stand the test of experiment and observation. Analogously, in the domain of formal deduction the Euclidean notion of self-evident axioms has to be replaced by the notion of postulates that are blanks with respect to truth-value, propositional forms rather than propositions. As the development of consistent non-Euclidean geometries proves, postulates are never necessary in the sense of having no conceivable alternatives. In the rationalistic tradition of epistemology, a priori principles are often spoken of as the fundamental *presuppositions* of all particular truths. But this view loses its impressiveness once the logical meaning of "presupposition" is clarified. Logically speaking, p presupposes q, if q is a necessary condition for p; and q is a necessary condition for p, if p implies q. Hence a proposition presupposes whatever propositions it implies, and the implications of propositions being indefinitely numerous, the logical relation of presupposing could not serve as a principle of selection of a small body of "a priori principles."

Postulates are freely *constructed* by the mind, they are not, like self-evident truths, *imposed* upon the mind. There is no *logical* compulsion to construct one set of postulates rather than an alternative, equally consistent, set; and even if such a set be constructed with a view to empirical application, the set which applies to empirical phenomena is, as Poincaré has shown with respect to geometry, by no means unique. But postulates express criteria which any phenomenon must satisfy if it is to be classified in a certain way; for example, the postulates of Euclidean geometry, including the theorems which they imply, prescribe the conditions which any figure must satisfy if it is to be said to be *really* a Euclidean triangle. The a priori is thus not *descriptive* of "essential structures," as the phenomenologists say, but *prescriptive* of veridical experience, or reality of a specific sort. Lewis' doctrine is Kantian in so far as the prescriptive function of a priori principles is emphasized and "mind" is viewed as the active function in scientific knowledge, not as a passive "tabula rasa" that is "imposed upon" by experience. There are, however, two main respects in which Lewis and Kant part company:

1) Like the Neo-Kantians, Lewis does away with the doctrine of "a priori forms of intuition." The *forming* or ordering activity of the mind is, according to Lewis, exclusively *conceptual*. In view of the fruitful application of non-Euclidean geometry (the Riemannian metric) to celestial mechanics, Lewis has to reject the doctrine of the "transcendental esthetic," according to which Euclidean geometry has no empirically applicable though logically conceivable alternatives, being expressive of the ways in which alone space can be intuited. The a priori does not limit the possible *presentational* content of experience. What is a priori are the mind's ways of *interpreting* the content of experience.

2) Granting that the knowledge of objects involves an a priori element, viz, the element of interpretation of the given, Lewis rejects, on the other hand, Kant's distinction between transcendental categories and empirical concepts. According to Kant, the transcendental categories, like substance and causality, give rise to a set of "synthetic a priori" principles. But Lewis takes no stock in the "synthetic a priori." The concepts under which the given is subsumed give rise to *analytic* if-then propositions, which explicate their meaning and state the conditions under which they may be said to have been correctly applied.

Specifically, these analytic conditionals state that x (the given particular) is such and such (falls into a given category) if it reacts in specifiable ways when certain operations are performed. All classification or

subsumption is thus *predictive*, and since its correctness depends on the verification of the implicit predictions, it expresses merely *probable* knowledge and is always "at the mercy of future experience." What is a priori and necessary are the conditionals which state the criteria of correct applications of concepts, since they are essentially analytic of what we *mean* by our class-terms. We can never be certain that x is S ; for the number of implied judgments which would first have to be verified is inexhaustible. All we can be certain of is, that *if* x is S , *then* x is P and Q , etc. Lewis' theory of the a priori may be conveniently summed up in the statement that the a priori is the *function* of conditionals to prescribe criteria of valid predication; in prescribing such criteria, conditionals define *reality of a sort*, that is, what is meant by a "real such and such."

On the one hand, Lewis insists on the analytic or definitive nature of these "criteria of reality." On the other hand, he claims that, since our classificatory judgments imply an indefinite number of other classificatory judgments in terms of which they may be verified, complete verification of our classifications is impossible and hence any subsumptive judgment is a merely probable hypothesis. But these two emphases are hardly compatible. Since any kind of empirical object may be exposed to an indefinite number of causal influences, its ways of reacting to stimuli are infinitely numerous. Each of these modes of reaction is *potentially* definitive of that kind of object; that is, the inquirer may *select* it as an essential or definitory property of the object. But, obviously, the number of such selected means of identification, whose totality constitutes the logical connotation of the general term that designates the defined object, is finite, even though the number of properties that may alternatively serve as *definiens*, is infinite. The greater part, therefore, of the conditionals which state that such properties are invariably connected with such other properties, do not explicate the *meaning* of a predicated class-term but express empirical laws and are thus, in the given context, synthetic. To illustrate. suppose we want to test the subsumptive judgment "this piece of wire is copper." Suppose copper is defined as an electrical conductor (omitting mention of the *differentiae* of the definition). Then the judgment "this piece of wire will be observed to conduct electrical current when it is connected with a battery" is a *logical* consequence of our initial classification. But the judgment "this piece of wire will be heated when connected with a battery" is, although formulating empirical *evidence* for the initial judgment, not a logical consequence of the latter. For the universal statement "electrical current heats the medium through which it

passes" is synthetic and empirical, unless, indeed, electromagnetic processes be *defined* in terms of their thermal effects. In short, the *potential* evidence for a subsumptive judgment, which is admittedly inexhaustible, must be distinguished from its actually *selected* evidence, which is necessarily finite. Accordingly not all of the conditionals which may function as "criteria of reality" can be said to be formally analytic.

Our approach to a priori knowledge in science differs from Lewis' "conceptual pragmatism" in so far as more emphasis is laid upon the actual making and functioning of the a priori—a posteriori distinction in scientific inquiry than upon the final formalization of this distinction in a well-defined and fixed language system. If the a priori is characterized in functional terms, it may well be viewed as susceptible of *degrees*. "All X are Y" is a priori, according to Lewis, if, finding that the concept Y does not apply to a given case, the inquirer retracts the concept X as likewise inapplicable. However, in order for such operations—in logical terminology: contrapositive inferences—to be performable, it is not necessary that "all X are Y" be analytic or true by definition; even though it is in the case of an analytically necessary judgment that X will be "retracted as inapplicable" with the least amount of hesitation, so to speak. The fact that the physicist is inclined to assume the presence of disturbing forces if he observes a certain body to accelerate, does not prove that the law of inertia is formally analytic; even though, conversely, if force is *defined* in terms of acceleration, the physicist is logically bound to draw such an inference. The distinction which Lewis (as well as Dewey, in the *Logic*) has failed to set forth with sufficient clarity is that between formal analyticity and analytic functioning (or, to coin a corresponding expression, "functional analyticity"). The former property is a sufficient but not a necessary condition for the latter property.

On the one hand, Lewis characterizes the a priori in *pragmatic* terms: "That is a priori which we can maintain in the face of all experience, come what will."¹ On the other hand, the identity of the a priori and the analytic or definitive is one of his basic contentions. Now, "can," in the statement quoted above, may refer either to logical possibility or to practical possibility. But in whichever of these two senses it be taken, Lewis' statement would hardly retain its plausibility if one were to substitute "analytic," in the sense of formal logic, for "a priori." Since, as Duhem has conclusively shown, it is never an isolated empirical hypothesis, but always an entire set of hypotheses that are submitted to experimental test, it is always *logically* possible to adhere to a given member of that set and to blame the failure of the deductive consequen-

ces to be verified upon the other hypotheses involved in the disappointed prediction; but such a possibility does not make an empirical hypothesis analytic. And if the possibility referred to is *practical* possibility, it would be extremely complicated to decide whether a given statement is analytic; at least this could not be decided by the methods of formal logic.

"In the case of an empirical law, a mere generalization from experience, if the particular experience does not fit it, so much the worse for the 'law' But in the case of the categorial principle, if experience does not fit it, then so much the worse for the experience."² With regard to actual scientific inquiry, so we shall contend, there is no fixed boundary line between "empirical laws" and "categorial principles." Empirical laws may *function like* analytic "categorial principles" without thereby foregoing their status of hypotheses that are open to empirical control. If we find a plot of ground which looks triangular but whose angle sum differs from 180° , the concept "Euclidean triangle" will be retracted as inapplicable to that plot of ground; the conditional "if anything is a Euclidean triangle, then its angle sum equals 180° " here proves itself a "categorial principle" which condemns a certain experience as illusory.³ It so happens that this conditional is an analytic theorem which renders explicit part of the implications of a mathematical concept. The present account emphasizes that such interpretative functions are exercised by *synthetic* conditionals, expressing inductive generalisations, as well. At the same time it follows Poincaré and Victor Lenzen in illustrating in terms of physical examples how inductive generalisations, in virtue of extensive empirical confirmation, are finally conventionalized and converted into analytic or definitive statements.

According to Lewis, a priori knowledge is expressed by the conditional statements in terms of which testable consequences are derived from the instantial propositions that *classify* the content of experience.

. . . The only kind of a priori knowledge of the empirical for which there is room in a consistent theory is that kind which consists in knowing the empirical eventualities, implicit in the application of our subject-concept, which are indispensable to the *correctness* of such application.⁴

But, clearly, the conditional statements that function as major premisses or rules of inference in the deduction of factual statements in terms of which one decides whether the initial classification was correct, need not be analytic; they may be empirical laws. In order to test whether a given particular is an instance of such and such a substance, one will

experimentally test its properties; the empirical laws which state that such are the properties of such substances are here the objects of "a priori knowledge" that supply the criteria for the correctness of our subsumptive "a posteriori knowledge." It should be evident from this reflection that a theory which identifies the a priori with an analytic function of conditionals determining the evidence in terms of which factual knowledge is to be tested, is hardly consistent with an identification of "a priori" and "formally analytic."

In *Appendix F of Mind and the World Order*, Lewis treats of the "logical correlates of the a priori and the a posteriori." A priori propositions are said to "coincide with the class of truths which are analytically determined and with propositions true in intension"; while "what is a posteriori coincides with the logically synthetic and with propositions true in extension."⁶ We shall meet an analogous emphasis on the distinction between "intensional" and "extensional" A-propositions in Dewey's doctrine of "universal" and "generic" propositions. An A-proposition is true in intension, if it is logically impossible, or ruled out by definition, that anything that is an instance of the subject-concept should fail to be an instance of the predicated concept, whereas it is true in extension if such a state of affairs simply happens not to be the case. It so happens that there are no white crows, but white crows would be quite conceivable. On the other hand, if we stick to the meanings of our terms, a non-rectangular square could not be consistently conceived. Now, in actual scientific inquiry, laws, expressed by universal sentences, are sometimes taken in extension, sometimes in intension. Just as the analytic or synthetic character of a sentence is relative to a specific language-system, with specific primitives and transformation rules, thus it depends on the context of experimental inquiry whether a law is true in intension, a priori, or true in extension, a posteriori. To illustrate: if one is interested in setting up a postulational formalization of dynamics, one will have to decide definitely whether equal weights are to be defined by Archimedes' law of the lever or by Hooke's law, or, in systematizing thermodynamics, one will have to decide definitely whether temperature is to be operationally defined by the law of thermal expansion, or Maxwell's law or Carnot's theorem (*via* the construction of a thermodynamic temperature scale) or some other law involving temperature as a variable. Once such a decision—which is conventional, in so far as alternative decisions are both theoretically and practically possible—has been made, the respective law will, in the respective system, be "true in intension," and correlatively other laws will be

“true in extension.” But in actual inquiry laws interchange their analytic functions, and provided such an interchange is not effected in one and the same inquiry, such that the inquiry gets caught in a vicious circle, this modal indeterminacy, so to speak, of scientific laws is harmless.

II. DEWEY'S DISTINCTION BETWEEN "UNIVERSAL" AND "GENERIC" PROPOSITIONS

AS THE kinship between Lewis' conceptual pragmatism and Dewey's instrumentalist theory of inquiry may lead one to expect, a methodological distinction analogous to Lewis' distinction between empirical laws and categorial principles underlies Dewey's theory of inquiry. Throughout his *Logic*, Dewey stresses the self-evolving and self-corrective character of inquiry. The standards or norms of inquiry evolve out of inquiry itself, Dewey urges. "Ways of operation" are formed and become formative with respect to future operations through successful performance of operations in the past; they do not call for extraneous sources such as "Bewusstsein ueberhaupt" or "conventions" stipulated by methodologists. The distinction to be discussed seems to be an outgrowth of this conception of a "continuum of inquiry."

"Universal" propositions are said to formulate modes of possible operations. Functionally speaking, they are plans guiding future experimental operations; genetically speaking, they formulate habits that have proved to be of instrumental value in the resolution of problematic situations. Such "universal" propositions are emphatically distinguished from "generic" propositions, which have the character of descriptive empirical laws. Since it must be acknowledged that "propositions" are entities analyzed by formal logicians, it seems appropriate first to examine Dewey's distinction in the light of formal logic.

From the standpoint of formal logic, one might be tempted to regard Dewey's distinction as equivalent to the distinction between a major premiss in a syllogism, and a rule of inference by means of which conclusions are inferred from major and minor premisses. Major premisses and rules of inference are, indeed, if one is to speak teleological language, "coordinate means" in the establishment of conclusions. The main profit that could be derived from such an interpretation is, that it would reveal the *contextual* character of Dewey's distinction. For what functions as a major premiss in one language, may function as a rule of inference in another language. For example, any theorem of algebra, such as the binomial theorem, may be a rule of inference with respect to arithmetic, but it is a conclusion and potential premiss within algebra. In general, all the assertions of pure mathematics are formal implications that may occur as major premisses in formal deductions, but their main use, from the standpoint of existential inquiry, just consists in their functioning

as rules of inference in the language of mathematical physics. Again, all the laws of nature have been called "rules" for the formation of "protocol sentences," i.e., elementary judgments of measurement, based upon pointer readings.* In the context of the language that is made up of "protocol sentences" (the language of experimental physics in the narrowest sense), they would thus be rules of inference; but they are undoubtedly employed as premisses in the language of theoretical physics. As Carnap points out, it is but a matter of stipulation whether the rules of inference (syntactical transformation rules) in physics are to consist only of analytic "L-rules" (i.e., logical or mathematical principles) or are to include also synthetic "P-rules" ("deskriptive Grundsätze"): "Es ist Sache der Festsetzung, ob man nur L-Bestimmungen oder auch P-Bestimmungen aufstellt; und die P-Bestimmungen koennen ebenso streng formal aufgestellt werden wie die L-Bestimmungen."† In fact, as a scientific language becomes formalized, what were originally conclusions and potential premisses tend to acquire the status of rules of inference. The fact that we employ a generalization as a rule of inference reflects our confidence in its reliability. When an inferred proposition turns out to be false, we will suspect the responsible black sheep to be among the premisses before questioning the validity of the rules of inference that we employed. Hence, if the distinctive mark of a "universal" proposition is its instrumentality as a rule of inference, we may say that any "generic" proposition tends to become a "universal" proposition as it is increasingly confirmed.

A "universal" proposition is said to express an "interrelation of characters" which is "necessary" and holds irrespective of existential exemplification of the interrelated characters; while a "generic" proposition is said to express a conjunction of "characteristics," in terms of which natural kinds are identified. Since we find it difficult to clarify what should be meant by the distinction between "character" and "characteristic"—except that the former occur in universal, the latter in generic propositions—we shall attempt to interpret Dewey's distinction in terms of methods of validation. A universal proposition, according to Dewey, is valid by "definition of a conception." Thus the proposition "all triangles are plane figures" would not, despite its deceptive extensional form, be validated by an examination of existing triangles; for it follows from the very definition of triangularity. On the other hand, Dewey cites propositions that would ordinarily be

*According to Schlick, only statements that are *definitely* decidable (i.e., verifiable or falsifiable) have the character of *assertions* (1). This view implies that there are *no general assertions*. All that can be said against this view is that it gratuitously violates a perfectly good usage of the term "assertion" ("Aussage").

regarded as empirical laws, like "if an animal is cetacean, it is mammalian," as instances of universal propositions. For the quoted statement of an inclusion of zoological kinds expresses, according to Dewey, "a necessary relation of characters and holds whether whales exist or not."³

Now, if "being mammalian" is a necessary condition and defining property (that is, a necessary condition *in virtue* of being a defining property) of "being a whale," the mentioned law of zoology may, indeed, be said to be valid "by definition of a conception" and thus to be a universal proposition. From the purely formal point of view, both this zoological law and the proposition about triangles would be analytic, in the sense that their negations are incompatible with the principle of non-contradiction. One feels, however, that the zoological law is not "non-existential" in the same way in which a mathematical proposition is admittedly non-existential. This feeling is grounded in the reflection that the proposition "if whale, then mammalian" was once a "generic" proposition (an empirical generalisation) which, on account of its reliability, has been transformed into a (partial) definition of the class-term "whale," and now functions as an analytical rule for identifying animals as whales: in so far as it is a universal proposition it is a "criterion of reality" in Lewis' sense; it helps us to determine whether a presented animal "really" is a whale.

Before proceeding, let us clarify just what is to be understood by an "analytic" proposition. An adequate definition of "analytic" should be free from Kant's restricted reference to propositions of subject-predicate form. The standard form of analytic truths is implication; as Leibniz pointed out, the "truths of reason" are always *conditional* in character. But if we define formal analyticity as characteristic of implications rather than of categorical statements, we must explicitly rule out Russell's "material" implications, defined in terms of truth-possibilities (a material implication is false if and only if the antecedent is true and the consequent is false). It is a necessary condition for the implication "for every x, if x is S, then x is P" to be valid, that "there is no x, such that x is S and x is not P." This condition is at the same time *sufficient* to define material implication. But in order to define *analytic* implication, we must introduce the modality "self-contradictory." "For every x, if x is S, then x is P" is valid as an analytic implication, if "there is an x, such that x is S and x is not P" is not only false, but implies, in terms of specified rules of inference, a contradiction. The process of converting empirical laws into definitional truths (to be illustrated in the sequel) could be formally reflected by a transition from

a material to an analytic implication (what Lewis calls "strict" implication). If analytic implications were based upon Aristotelian "real" definitions that are beyond possibility of revision, the implication "for every x , if x is S then x is P " would rule out once and for all the possibility that there should be found an x which is S but not P . From the above implication and the fact that a is not P , we could infer, without empirical investigations, that a is not S . But, actually, our confidence in the empirical law "all S are P ," which led us to define S in terms of P and thus to treat the universal sentence as a logical truth, may have been premature. Finding an instance that *appears* as S but does not exhibit P , we may decide to abandon our analytic implication as an inapplicable rule, instead of concluding that the problematic instance is not a *real S*.

When Dewey accuses modern treatises on logic to confuse different types of A-propositions, he may have in mind just this distinction between synthetic and analytic universal propositions. In fact, *Principia* is an "extensional" logic, and modal distinctions cannot be expressed in an extensional logic except meta-linguistically,* hence the symbolism of *Principia* does not enable us to distinguish between contingent (synthetic) and analytically necessary A-propositions. In accordance with the traditional "square of opposition," "(for every x , if x is S then x is P) implies (there is no x , such that x is S and x is not P)"† is valid, in the system of Whitehead and Russell, by the very definition of "implication." Yet, consider the two A-propositions "all crows are black" and "all squares are rectangular." Either one would be symbolized, in the symbolism of *Principia*, with the help of the familiar "horseshoe," to express the logical constant "if, then." Consequently, by the very definition of the horseshoe-symbol, "all crows are black" would be equivalent to the negative existential statement "there are not any non-black crows," and "all squares are rectangles" to "there are not any non-rectangular squares." Obviously, the "there are not" clause has

*Thus Russell gives the following meta-linguistic analysis of the meaning of "possible": "if ' ϕx and χx ' and ' ϕx and not χx ' are each true for suitable values of x , then, given ϕx , χx is possible but not necessary" (Inquiry into Meaning and Truth, New York 1940, p. 43).

†Notice that "(for every x , if x is S then x is P) implies (there is an x , such that x is S and x is P)" is invalid, in *Principia*. In Aristotelian logic, on the other hand, the inference from the A-proposition to the I-proposition (subalternation) is recognized as valid. A logic which would formally take account of the distinction between existential or synthetic and non-existential or analytic A-propositions, should stipulate that subalternation and conversion by limitation (if all S are P , then some P are S) are valid if applied to synthetic A-propositions (inductive generalisations) and invalid if applied to analytic A-propositions. As has been pointed out, the fact that subalternation is valid in Aristotelian logic is indicative of the *ontological* import of the Aristotelian notion of *classes*.

different logical force in the two examples: in the former example, it is asserted that such *is not the case*, in the latter example it is meant that such *cannot be the case*.

In Lewis' modal logic, analytic entailment ("strict" implication) can be symbolically differentiated from extensionally defined material implication. Now, in a still more differentiated symbolic language it should be possible to symbolically distinguish, not only "material" and "strict" implication, but moreover different kinds of analyticity: an analytic implication that is based on an empirically grounded definition (or, conversely speaking, a conventionalized empirical law) should be distinguishable from an analytic implication that is based on a mathematical definition. Otherwise, if "being a triangle implies being a plane figure" and "being a whale implies being a mammal" are indiscriminately referred to as "universal" propositions in the sense of being necessary by "definition of a conception," an important difference is neglected: for the latter proposition is analytic relatively to a definition which embodies an inductive generalisation.

When a "constant conjunction" (to employ Hume's terminology) expressed by a generic proposition of the form "when and where A, then and there B" is *explained* by the interposition of a middle term C, it may appear as a "necessary connexion" expressed by the propositional form "*if A, then B*" If the premisses from which it is inferrable—or rather, to render the order of inquiry adequately, in terms of which it is explained—have a considerable degree of generality, it is, as it were, made the associate of a whole class of other empirical laws which follow from the same premisses. Once an empirical law is incorporated into such a class of empirical laws which may be simultaneously explained by a single comprehensive theory, we feel a certain *pragmatic* necessity of abiding by its validity: for we are most reluctant to abandon a theory which unifies a considerable body of empirical laws. Yet, such a unifying theory is itself synthetic (in the sense of containing at least one synthetic proposition), and hence the particular laws which it explains are still synthetic and open to nullification by experience. This may be illustrated by the kinetic theory of gases which enables one to express the empirically discovered functional relations between the thermodynamic state variables in terms of relations between mechanical average quantities. From the assumptions of kinetic theory, Boyle's law may be statistically derived; the "fact" that at constant temperature the product of pressure times volume of an ideal gas is constant, is thereby converted into a "reasoned fact." Using a distinction of Bosanquet's, we might say that the functional relation expressed by Boyle's law has

been converted from a "*de facto* conjunction" into a "*de jure* connection" This conversion, however, could be effected only with the help of another "*de facto* conjunction," viz., the proportionality of the temperature of a gas to the average kinetic energy of a gas molecule, expressed by Maxwell's law To be sure, if all that the explanation of empirical laws by theories amounted to were the substitution of one "*de facto* conjunction" for another, there would be no inherent advantage in such explanations But a fruitful theory, as contradistinguished from an *ad hoc* hypothesis, transforms at once *several* "facts" into "reasoned facts." The theory of gravitation explains not only Kepler's laws of planetary motion, but also Galileo's law of freely falling bodies and the phenomena of the tides; the kinetic theory explains not only Boyle's law and the law of Gay-Lussac, but also the law of Avogadro, Dalton's law of partial pressures, etc.

A-propositions may be formulated in categorical or in hypothetical form. What is asserted by "all S are P" is logically precisely the same as what is asserted by the conditional sentence "for every x, if x is S, then x is P," even though a translation from categorical into hypothetical form is often accompanied by a transformation of nouns into adjectives. If, however, the context of inquiry in which A-propositions are formulated is kept in view, this difference of formulation may be correlated with a difference of motives If I say "all metals are good electric conductors" I am, as a rule, stating the result of an inquiry If I say, instead, "if this is a metal, then it is a good electric conductor," it is likely that I am engaged in a process of solving a problem and am *using* the above generalisation as a criterion for determining whether a given singular falls into the kind of metals. In so far as this sort of a use is made of an A-proposition, it may, in that context of inquiry, be said to be functionally analytic Dewey's treatment of the distinction between "generic" and "universal" A-propositions leaves one with the impression that there is a class of A-propositions which are *inherently* "universal" in the sense of formulating methods of solving experimental problems The present analysis treats "universality" in Dewey's sense as a *functional* property which A-propositions may have, no matter whether they be logically analytic or synthetic Whenever a hypothesis, in the form of an A-proposition, is to be experimentally tested through application to concrete instances, it is recommendable to formulate it with explicit reference to experimental operations It will then assume the form of a conditional sentence whose antecedent formulates possible operations and whose consequent predicts the observable effects of those operations Since, according to Dewey, "the universal hypothetical states the re-

lation between the operation and its consequences . . . ,”⁶ it follows that any empirical hypothesis can be construed as a “universal” proposition, provided its *operational* meaning is made explicit, i.e., it is stated in such a way as to explicitly indicate a method of its verification. In its operational formulation, a hypothesis will usually have the structure: p implies that if q, then r, where p refers to the property that is hypothetically predicated and whose presence is to be tested, q to the testing operation, and r to the expected effects of the operation, whose occurrence verifies the predication of p. If the hypothesis is expressed with the purpose to *assert* an invariant relation or a *uniformity*, the mention of q will usually be omitted, and it could then hardly be said to formulate a “possible mode of operation” and thus to be “universal.” If, however, it is expressed with the purpose to *direct* experimental operations, q will appear in its formulation, and it will be “universal” or “a priori” in *function*.

III. THE TRANSFORMATION OF INDUCTIVE GENERALISATIONS INTO DEFINITIONS

THERE IS a tendency, especially among idealist logicians (such as Bosanquet), to mark out so-called "intensional" judgments, whose sentential expressions are characterized by the occurrence of abstract nouns (the linguistic sources of Platonic Ideas), as standing on a higher plane of necessity than mere inductive generalisations that predicate a property of "all" of a certain kind. Dewey himself seems to have been influenced by this trend of logical theory, as may be surmised from his insistence on the difference between the necessary universal proposition "man is mortal"—to pick out the immortal textbook illustration—and the mere generalisation "all men are mortal."

If pressed to explain what is meant by saying that humanity and mortality are *necessarily* connected attributes, such a logician is likely to reply that mortality is of the *essence* of humans. Yet, that mortality is of the *essence* of man is merely the belief that the inductive generalisation "all men are mortal" will never be contradicted by experience. We may become so confident in the validity of such a generalisation that we *hesitate* to call an immortal being "human", in so far our generalisation functions analytically. If we should even decide to *define* "human" in terms of "mortal," our generalisation will have become formally analytic, and mortality will be essential to men in the sense of forming part of the logical connotation of the term "man."

There is an obvious sense in which such a definition is—for lack of a less ambiguous term—"grounded" in matters of fact, unlike an arbitrary stipulation concerning a synonymous use of two expressions. The construction of many definitions is primarily motivated by the desire to simplify or economize our linguistic symbolism. Their genesis may be schematized as follows. a concept has been constructed out of elements a,b,c which are designated by terms whose meaning is already familiar to us; we then decide to introduce, for the sake of verbal abbreviation, a term D, standing for the concept a,b,c. This type of definition, which may be called *nominal*, is exemplified by the definition of a "spinster" as an unmarried, adult female. Loosely speaking, we might say that the construction of such definitions proceeds from *definiendes* to *definiendum*. Most mathematical definitions, such as the definition of 'i' as the

square root of -1 , are of this kind. On the other hand, it often happens that the *definiendum* is a term having already more or less unambiguously identifiable denotative referents; that is, we are already acquainted with the objects to which it is properly applied. We then set ourselves the task to discover a set of common properties of the denoted referents, which may serve as the logical connotation, the *definiens*, of the term. Thus the definition of "whale" in terms of mammalian traits grew out of the empirical discovery that the supposed fish exhibit the defining traits of mammals. Such definitions, which are the result of empirical investigations, may be called *factually grounded* or *real*. Their essential function is not so much to economize language than to serve, as will be illustrated presently, as criteria of valid classification.

Before developing our theme further, let us consider a classical objection to the notion of real definition, raised by Mill in his *System of Logic*. According to Mill, the belief in real definitions, which propagated itself from Aristotle's *Posterior Analytics* throughout the middle ages, until it called forth Hobbes' nominalistic reaction, arises from a confusion of nominal definitions with existence postulates. What claims to be a real definition is resolvable, according to Mill, into the assertion of the existence of a kind of entity—and this assertion it is which is either true or false—and the arbitrary assignment of a name to that kind.

There is a real distinction, then, between definitions of names, and what are erroneously called definitions of things; but it is, that the latter, along with the meaning of a name, covertly asserts a matter of fact. This covert assertion is not a definition, but a postulate. The definition is a mere identical proposition, which gives information only about the use of language, and from which no conclusions affecting matters of fact can possibly be drawn. The accompanying postulate, on the other hand, affirms a fact which may lead to consequences of every degree of importance. It affirms the actual or possible existence of things possessing the combination of attributes set forth in the definition, and this, if true, may be foundation sufficient on which to build a whole fabric of scientific truth.¹

Mill's proposed separation of cognitive assertions, capable of serving as premisses, and definitions, seems quite plausible. It must, indeed, be admitted that such a separation is feasible in many cases. If, for example, Hooke's law, which may function as a premiss in physical deductions, should be claimed to be a real definition of a certain kind of physical constant, viz., a modulus of elasticity, one could clearly separate the existential judgment that constant ratios of stress to strain exist, from the convention to *call* such constant ratios "moduli of elasticity."

Yet, as Mill's statement that a definition "is a mere identical proposition" indicates, the sort of definition he tacitly referred to is the traditional *explicit* definition, with the *definiendum* at the left hand side of the definitional equation, and the *definiens*, composed of *genus* and *differentia*, at the right hand side. Mill was not acquainted with the modern technique of *definition by postulates*, which type of definition is more current in mathematical sciences like physics, while the scholastic type of explicit definition *per genus et differentiam* is more current in classificatory sciences like botany or zoology. Now, an explicit definition may always be construed as a meta-linguistic rule permitting the replacement of a set of symbols (the *definiens*) by a less complex symbol (the *definiendum*), and thus simplifying the language of a science. An implicit definition (definition by postulates), on the other hand, consists of postulates serving as premisses in the *object-language* of a science * In this respect implicit definitions, consisting of postulates from which consequences are derived, are *real* definitions in the sense in which Mill denied that there are any such definitions; even though Mill was right in maintaining that no conclusions "affecting matters of fact" can be derived from mere definitions, that is, in order to derive empirically testable consequences from such postulates one has to supplement the latter with statements of boundary conditions.

Postulates may be said to function as implicit definitions of classes of

*The following are properties which unambiguously differentiate implicit and explicit definitions 1) An explicitly defined term is, by the rule of the substitutability of equivalents, *eliminable*, while an implicitly defined term is not 2) One usually distinguishes between object-linguistic axioms and explicit definitions which are *rules about* the use of terms in the object-language and in so far meta-linguistic statements Implicit definitions, however, *are* constituted by axioms in the object-language, they are not statements *about* the use of *definienda*, but rather *exhibit* such usage 3) An explicit definition determines a *unique* type of object or class-concept, whereas an implicit definition determines an entire class of concepts and does not by itself—that is, without being supplemented by "co-ordinating" definitions—warrant that this class be a unit-class The primitive notions, which are implicitly defined by a set of postulates, are variables: they, to use Russell's phrase, "ambiguously denote" possible interpretations or "verifiers" This feature may be illustrated by two of Hilbert's "axioms of connection," which may be regarded as constituting an implicit definition of the primitive notions "point" and "straight line." 1. Zu zwei Punkten A,B gibt es stets eine Gerade a, die mit jedem der beiden Punkte A,B zusammengehört 2. Zu zwei Punkten A,B gibt es *nicht mehr als* eine Gerade, die mit jedem der beiden Punkte A,B zusammengehört In short two points determine one and only one straight line Now, in so far as the primitive notions are variables and must not be confused with their possible interpretations, the concrete Euclidean terms "point" and "straight line" had better be replaced by the variable letters 'x,' 'y,' 'z' The following, then, are possible interpretations of the propositional function $xyRz$ (in which the only constant is R, standing for the relation of unique determination) two Euclidean points uniquely determine a Euclidean straight line, two Euclidean straight lines uniquely determine a Euclidean point Again, x and y may be interpreted as ordered pairs of real numbers and z as a linear equation the axioms of Hilbert's pure geometry would then be converted into axioms of Cartesian or analytic geometry.

entities in so far as they are used as "criteria of reality," i.e., criteria of valid classification.* In order to determine whether the class implicitly defined by such postulates is the null-class or contains members, one has to verify whether entities exist with respect to which the postulates are true. Thus one might define *absolute space* as any reference-frame with respect to which the Newtonian law of inertia is true. The question whether absolute space exists would, then, have a perfectly good operational meaning: it would amount to the question whether there exists a reference frame such that, in terms of defined methods of measuring length and time, an approximately isolated body moves approximately without acceleration relatively to that reference frame. In an analogous fashion, and in somewhat greater detail, the class of absolute motions (or the concept of absolute motion) may be implicitly defined by the following postulates:²

1) If one knows, at a given instant, the initial conditions of particles $M_1, M_2 \dots M_n$, the absolute motion of these particles is uniquely predictable in terms of differential equations of the second order (principle of determinism)

2) If at two instants t_0 and t_1 the same initial conditions are realised, with the sole difference of the place and time of their realization, the same absolute motion, relatively to a fixed reference frame, will be observed ("same cause, same effects," applied to mechanics).

3) The absolute motion of a particle M is not modified if one changes, in any manner whatsoever, the positions and velocities of the particles sufficiently distant from M , without changing the initial conditions of the particles lying within the causal range of M (principle of closure) † The question whether absolute motion exists is, in terms of this implicit definition of absolute motion, operationally significant; it is synonymous with the question whether there exist particles and reference frames with respect to which the above postulates are true.

In Newton's *Principia*, which are for mechanics what Euclid's *Elements* are for geometry, one finds a neat separation of definitions and axioms. The artificiality of this separation has been revealed by Mach,

*The reader should bear in mind the distinction, intimated in section I, between the perspective of the formal logician who organizes acquired scientific knowledge into a definite system, and the perspective of the scientist for whom scientific knowledge is in the making. In actual inquiry, inductive generalisations function definitionally or analytically without thereby foregoing accessibility to empirical test in other contexts. If, however, an inductive generalisation is, in the process of systematization, definitely converted into a definition, it is thereby eliminated from the body of empirical propositions, and the fact that, as a *real* definition, it is *genetically* based upon empirical discoveries, does not make it *logically* dependent on empirical facts.

†cf Poincaré, *Les Principes de la Mécanique*.

in his critical and historical analysis of the principles of mechanics. For example, as will be shown in detail,* once Newton's definition of force is accepted, the corresponding axiom (the second law of motion) is true *by definition*. The appearance of the axioms of an existential science, such as mechanics, as definitional truths derives from the fact that the order of inductive discovery is, as it were, the inverse of the order of systematic or deductive exposition. That is, when a science outgrows its "natural history stage" and reaches its deductive stage, inductive generalisations are transformed into postulates which implicitly define the fundamental concepts of the science.

This method of definition by postulates is analogous to the axiomatic method employed by Hilbert in his reconstruction of Euclidean geometry. In Euclid, a point, for example, is *explicitly* defined as that "which has no parts." In Hilbert's system, the axioms which describe relations between the elements of the classes that are denoted by the primitive terms "point," "straight line," "plane," *are* the definitions of the primitive notions. By this method of implicit definition, these geometrical notions are, indeed, deprived of all *intuitable* content; a "point" may just as well mean an ordered triplet of real numbers as the intersection of two free light rays. We find, however, an analogous conceptualization in physics, where a "wave," for example, is defined as any solution of a certain type of differential equation, such that the concept of a wave comes to be applied to phenomena that were not originally denoted by the concrete term "wave." Thus Maxwell's equations of the electromagnetic field are satisfied by light as well as by electricity; in this respect they constitute an implicit definition of a class of isomorphic classes in an analogous way in which Hilbert's axioms are said to "define."

The concept of implicit definition may be objected to, on the ground that it is confusing terminology to call postulates "definitions." But an examination of scientific uses of the term "definition" would probably disclose that a necessary condition to be satisfied by a definition is its ability to function as a criterion of valid predication (classification). Of course, there are, as a rule, several criteria of valid predication of a concept available, and hence this capacity to function as a criterion is only a necessary, not a sufficient condition for a proposition to function definitionally. For example, a body is in equilibrium if and only if $\Sigma F = 0$ and $\Sigma L = 0$ (where 'L' stands for torque or moment of force); also, a rigid body is in equilibrium if and only if it does not undergo any linear or angular acceleration. As a matter of fact, in virtue of

*cf. part two, section I, 2.

Newton's second laws for linear and angular motion (proportionality of force to linear acceleration, proportionality of torque to angular acceleration), these two criteria of equilibrium are mutually equivalent, i.e., if $\Sigma F = 0$ and $\Sigma L = 0$ are satisfied, then the rigid body does not undergo acceleration, and conversely. Hence, if capacity to function as a criterion of the validity of a predication of the form " x is P " (where the nature of the admissible values of x is determined by the nature of the predicate)* were a *sufficient* condition for a proposition to be a definition, we would have several definitions for one and the same concept at the same time. Obviously, which of these potential criteria be selected as the actual definition of a concept involved in all of them, depends on the objectives of inquiry; in this sense the distinction between definitional and empirical truth may be said to be relative to a functional context. Thus, if one is engaged in experimental verification of the principle of equilibrium, one will define equilibrium as a state of non-acceleration. But once the principle of equilibrium is experimentally established, it may henceforth be used as a "criterion of reality": for the safest way to determine whether a given rigid body is "really" in equilibrium is to measure the forces and torques acting upon it, and see whether they satisfy, within the limits of experimental error, the conditions $\Sigma F = 0$ and $\Sigma L = 0$.

The conversion of the principle of equilibrium from an experimental truth into a definition of equilibrium is intended to illustrate how an empirical law may acquire the function of a definitional "criterion of reality" when it has risen to the status of an extensively confirmed hypothesis. This process towards analyticity is at the same time a process of refining or articulating empirical concepts. To establish the principle of statics stated above as a *synthetic* proposition, one had to understand roughly what is meant by "equilibrium"; as we might put it, the meaning of this term was "denotatively" clear;† one knew in a pre-analytic way what kind of situations it is proper to designate as cases of equilibrium, without being able to state a quantitative *criterion* of equilibrium. Through the establishment of the principle of equilibrium the concept of equilibrium became post-analytic, articulate, or well defined. Analogously, when Newton established the third law of motion, according to which there corresponds to every "action" an equal and opposite "reaction," he had to possess an intuitive under-

*It may be said in general that, in a postulational system, the nature of the non-logical constants occurring in the postulates delimits the domain of application of the postulates to a definite kind of entities. It is the differentiating feature of *logical* postulates that they do not contain non-logical constants and are hence applicable to *everything*.

†cf pp. 15-16.

standing of what is meant by "mass" (the definition " $m = F/a$ " would do only if force could be defined without reference to mass); this intuitive notion of mass is expressed in the characterization of mass, as it may be found in elementary treatises on physics, as the "measure of inertia, i.e., the tendency of a body to resist changes of state." Mach, then, used Newton's third law to refine the concept of mass, in the sense of rendering mass measurable by kinetic methods

As the *motto* which is prefixed to this essay indicates, Poincaré's "conventions" (in mechanics) are principles that have been "derived" from experimental laws—in much the same way as our "real definitions" are thus derived—at the same time, however, have been immunized against possible invalidation by future experience. They function like Lewis' "categorical principles," in so far as, if an experience does not fit them, "so much the worse for the experience." Our emphasis on this conventionalization of inductive generalisations has been greatly influenced by Victor Lenzen's detailed application of Poincaré's notion of "conventions" to physical theory. Lenzen is Kantian in so far as he acknowledges that universal principles enter as essential determinants into what the physicist declares as "reality." These "constitutive conditions of experience," however, are, for Lenzen, "synthetic a priori" only in the crooked sense of being, on the one hand, empirically grounded, and on the other hand, a priori in their "constructive function." For example, the principle of the conservation of mechanical energy strictly applies only to reversible frictionless processes. It is an empirical fact that the energy of a closed system, defined as the sum of a certain function of position (the potential energy) and a certain quadratic function of velocity (the kinetic energy) is constant in time, but varies if the system does work against friction. Through the discovery of the mechanical equivalent of heat, however, the concept of energy was redefined as a sum of three terms in such a way that the property of conservation could be predicated of energy, no matter whether reversible or irreversible (thermodynamic) processes were concerned. As Lenzen shows, this "constructive function" of the energy principle is also transparent in contemporary subatomic physics. The phenomena of nuclear disintegration (emission of beta particles from radioactive nuclei) seem to present an exception to the conservation principle. But in order to save the unrestricted applicability of the principle, physicists have postulated the existence of "neutrino" particles. "A neutrino is assumed to participate in the emission of a beta particle so that there is conservation of momentum and energy. The new entity is required in order to preserve the universality of the principles of conservation."⁴

In illustrating the transformation of empirical laws into definitions (the "abstractive function" of reason) and the function of definitions as tools for the discovery of further empirical laws, Lenzen refers to the "method of successive approximation" and the "method of successive definition"*. Actually, these are not different methods, but "successive approximation" refers to a characteristic property of the process of successive definition. This may best be elucidated by example. All measurement of length presupposes the choice of a standard rod. Since all variations of length are determined by comparison with the length of this standard, it is operationally meaningless to ask whether the standard is invariant in length: its length is *defined* as invariant. Still, it is observed that the results of measurements of length, performed with the standard rod, vary with variations of temperature. But since a definition of a temperature scale presupposes a definition of length, it would seem to be circular to define a standard rod with reference to a definite temperature. This vicious circle may, however, be broken, by distinguishing between degrees of approximation. With the help of a vaguely defined standard rod one may construct a temperature scale which serves to define temperature to the first approximation. So far one had merely known that temperature is *a* function of length; the *form* of the function was unknown. But now, with the help of a thermometer, one discovers the law of thermal expansion according to which volume or length is a linear function of temperature. This law in turn may be used to correct errors in measurements of length due to temperature fluctuations, and to define a standard rod to the second approximation. The law of thermal expansion, established by means of inaccurate methods of measuring length, is a fairly good approximation, owing to the fact that changes of temperature cause but very minute changes of length of a solid rod †.

There is, theoretically, no end to this dialectical process of successive approximation to ever more accurate laws. Thus, estimating the equality of two weights crudely in terms of the gravitational pull felt in one's muscles, one may experimentally establish the law of the lever. This very law defines a more accurate method of determining equality of weights. In terms of this quantitative criterion of equality of weight one may in turn verify a law, such as Hooke's law, which may serve as a still more accurate "conceptual tool" for comparing weights.

The process of "successive definition," which Lenzen illustrates, in

*cf *Procedures of Empirical Science*, International Encyclopedia of Unified Science, I, III, 13, 14

†cf *Procedures of Empirical Science*, pp. 11-12.

The Nature of Physical Theory, by examples drawn from all the various branches of physics, involves a delicate epistemological problem. In which sense can a concept that has been redefined in terms of a generalisation be said to be still the *same* as the original, more particularized concept? If objects that are subsumable under *different* concepts may compose identical aggregates (a state of affairs that corresponds to any valid equivalence which claims to be non-verbal), how are we to decide whether such concepts whose extensions are identical are "really" different? May be that the difference resides merely in the names by which these equivalent concepts are designated? Conversely, if name, concept and extension are distinct (though possibly inseparable), what warrant do we have to infer identity of concept from identity of name and extension? Surely, it may happen that a name changes its intension (logical meaning) while its extension does not undergo any concomitant variations. If, for example, one philosopher defines man as a rational animal, another as a symbolizing animal, a third one (from the Augustinian school) as a fallen angel, it would seem that their *concepts* of man differ, although they use the same term "man" and *apply* that term to the same individuals. The only safe position one can take in this matter is to say that a concept *is* what it is *defined* to be. Accordingly we have to say that we obtain different concepts whenever we redefine our general terms, even though the denoted referents of the latter remain the same (numerically); unless, indeed, the novel definition can be shown to be equivalent to the former definition which it replaces.

This decision concerning the use of the term "concept" implies that it is not non-linguistic propositions that may, in the progress of inquiry, shift from the status of synthetic empirical truths to the status of analytic truths, but sentences. For, suppose the proposition which is said to be at one time synthetic and at another time analytic, has subject-predicate form. Suppose the concept upon whose instances predications are made, and which may be symbolized by "A," is at time t_0 defined by properties b, c , such that at time t_0 the proposition "A is d" is synthetic. Now you convert this empirically discovered property d into one of the defining properties of A; hence, at time t_1 , let us say, the proposition "A is d" is analytic. Yet, it is illusory to think it is the *same* proposition which at time t_0 is synthetic and at time t_1 analytic; in truth it is only the sentential expression that has remained the same. For a substitution of *definiendum* for *definiens* would reveal that the *concept* which "A" designates at t_0 is different from the concept which it designates at t_1 . Hence, if this conceptual difference were reflected in the sentential expression, it would be obvious that the synthetic

proposition asserted at t_0 (A is d) is different from the analytic proposition asserted at t_1 (A' is d).

If a statement is said to be analytic in virtue of the fact that its truth follows from the very meaning of its terms, it is clear anyway that "analytic" is a predicate qualifying linguistic sentences, not non-linguistic propositions expressed by sentences. Our contention that scientific truths undergo a development from contingency to analytic necessity (in Poincaré's language, experimental laws are "erected" into "conventions") might then be reformulated as follows: one and the same sentence which is in one context of inquiry synthetic may, in a different context of inquiry, be analytic, in virtue of a shift of *meaning* undergone by some of its terms.

A most common form which this process takes is generalisation by selective abstraction. The instances of a concrete concept are empirically discovered to have a certain common property, which is then selected as the *definiens* of the term connoting the concrete concept. In this way, to illustrate, acceleration became the defining characteristic of force in Newtonian mechanics. But now one finds that the defining concept has a greater range of applicability than the defined concept: acceleration is observable in many situations—such as planetary motions—in which forces, as factors experienced in the "mode of causal efficacy" (to speak Whitehead's idiom), are not observable. The *definiendum* and the *definiens* are then tacitly rendered coextensive by abstracting from the elements of concrete meaning, originally connoted by the defined term, which inconveniently delimit the range of applicability of the defined term. When we attach a heavy object to one end of a string, and whirl the object around in a circle, we *feel* the centripetal force which is said to keep the object in a circle as a muscular strain. But if we imposed the feeling of strain or tension as a limiting condition upon the applicability of the term "force," we could not extend the concept of centripetal force to planetary motions, since only the sun herself could tell whether she feels a tension when the planets move around her (besides, her testimony may not even be worth considering, since the planetary revolutions are presumably uninterrupted and, as Hobbes said, "a sensation that is always felt, is never felt"). Thus, the term "force" being purged of its original "animistic" elements of meaning, the explanation of change of tangential velocity in terms of the action of a centripetal force becomes tautological, since there is no other way of verifying the presence of such a "cause of change of motion" except the *kinematic* way of observing the effected change of motion.*

*Without going into details, it may be suggested that Einstein's "principle of equivalence," in virtue of which gravitational fields of force may be "generated" or "trans-

At this point the cynical reader may feel tempted to shrug his shoulders and say: is it not making a rather trivial point to contend that the meanings of the terms of a sentence may be changed in such a way that relatively to one set of definitions the sentence is synthetic, whereas relatively to another set of definitions it is analytic? Still the matter is not as trivial as that: for the sort of redefinitions which we, following Lenzen's theory of "successive definition," refer to, are not arbitrary but grounded in empirical discoveries. In particular, we are referring to redefinitions which leave at least part of the extension or denotation of the redefined term unchanged what is changed are not so much the objects conceived as our ways of conceiving of the (numerically) same objects. Chemical elements were first defined in terms of "secondary qualities"; relatively to these concrete definitions, the statements that such are the atomic weights of such elements were synthetic. At a later, more analytic, stage of chemistry, chemical elements were defined in terms of their atomic weights, thus the formerly synthetic statements became analytic. In modern electronic theory, chemical affinities are "reduced" to numbers of "free" electrons in the ionized atom, and the "nature" of chemical affinity is said to consist in the attractive forces exerted between the electronic constituents of charged atoms. Thus the scientist's ways of conceiving of his entities undergo, under the pressure of empirical discoveries, a constant change. But what lends *continuity* to this process of empirically grounded redefinition is the fact that it is the numerically identical objects that satisfy all of these successive definitions. What usually happens, though, is that these successive definitions *generalize* the original concept, i.e., extend its domain of application. The developments of the concepts of force and energy exemplify this process of generalisation.

In general, scientific laws of conditional form tend to become analytic of the nature of their subject-matter, as they become increasingly confirmed. Thus, physicists experimentally ascertained that some gases are such that their liquefaction requires the application of enormous pressures. They extrapolated and constructed the concept of an *ideal* gas, as a gas which, owing to absence of intermolecular forces, is in principle incapable of liquefaction. But after the discovery of Boyle's law, the physicist is prone to define an ideal gas as any gas which satis-

formed away" by a mere interchange of inertial and accelerated reference frames, is shocking to common sense just because the term "force," which is intended by relativists in the sense in which the statement of the proportionality of force to acceleration is analytic, calls up in the unsophisticated mind the original anthropomorphic meaning in which the same statement is synthetic (cf. Kasimir Ajdukiewicz, *Das Weltbild und die Begriffsapparatur*, esp. § 2, in *Erkenntnis*, vol. 4)

fies Boyle's law. Indeed, as was illustrated by the use made of the conservation principles in subatomic physics (cf p. 21), physical theories may even be used to postulate entities which render them applicable. *Unobservable* entities are often introduced into physics, in order to support theories whose deductive consequences describe *observable* phenomena. These theories constitute implicit definitions of the unobservable entities, and the question whether these implicitly defined entities exist means, operationally, no more than whether the deductive consequences of the implicit definitions (the theories) check with the observable phenomena. Thus the ether was introduced into the physical universe as an unobservable elastic and continuous medium, in order to render the undulatory theory of light compatible with the inherited notion of a wave as a "disturbance propagating itself with finite velocity in a *medium*." The differential equations of wave motion, then, constitute an implicit definition of the ether, inasmuch as the ether is the postulated "substratum" of the kind of motions that satisfy these differential equations. Analogously, the point-particles of Newtonian dynamics are postulated unobservable entities whose essential or definitory properties are inertia and gravity (although in Newton's own opinion only inertia was an essential property); inertia, however, is implicitly defined by the Newtonian laws of motion and gravity is implicitly defined by the law of gravitation *

With regard to existential inquiry, analyticity is an ideal limit. The concepts "analytic" and "synthetic" are no doubt mutually exclusive, but if scientific inquiry is viewed as a continuous process, it will be seen that empirical laws converge from the status of synthetic descriptions towards the status of analytic "criteria of reality" (it being understood, though, that "criteria of reality" have no *final* status but are *instrumental* in further inquiry). If it is permitted to draw an analogy from the pre-scientific level of behavior, this convergence towards analyticity appears in rudimentary form, as it were, in the process of conditioning. If two stimuli S and S' have been repeatedly associated in an individual's experience, S will become a *sign* of S' or, conversely speaking, S' will

*If a science aims at the establishment of fundamental laws, and the fundamental laws of a science implicitly *define* its subject-matter, are we not driven to the paradoxical conclusion that science is a search for subject-matter? The paradox is easily dispelled by distinguishing between subject-matter known by qualitative acquaintance and subject-matter known by scientific description. Science discovers the properties of a subject-matter given in experience, and then *defines* the subject-matter in terms of its discovered properties. Science differs from mathematics in that its precise definitions come at the *end* of an inquiry. The inductive genesis of scientific definitions is disguised by the fact that in the deductive exposition of an inductive science the definitions appear as "primi-

become the *meaning* of S. The individual will tend to respond directly to S', as an anticipated consequence, and cease to be consciously aware of the immediate stimulus S. The analogue of such *habits*, manifested in conditioned responses to causally signified meanings, on the level of scientific discourse, are conditional if-then propositions. The stage of inquiry where such a proposition is synthetic, corresponds to the phase of mediate response to S': S and S' form a "constant conjunction," but the individual responds to S and S' in two separate acts of awareness. The tendency towards analyticity, i.e., the tendency of the consequent of a conditional proposition to express the *meaning* of the concept hypothetically predicated in the antecedent, corresponds to the formation of a habit.

IV. THE ANALYTIC FUNCTIONING OF EMPIRICAL LAWS

IT is the purpose of this section to illustrate how empirical laws function, in certain contexts, analytically, without being on that account irrefutably a priori. We repeat that while formal analyticity is a sufficient condition for analytic functioning, in the sense to be illustrated presently, it is not a necessary condition. What renders both Dewey's doctrine of the "universal" proposition and Lewis' doctrine of "categorical principles" ambiguous or misleading, is just that sometimes these "a priori" propositions are characterized as being analytically necessary, and sometimes they are marked off from inductive generalisations in terms of a way of functioning of which, as will be shown, inductive generalisations themselves are capable

As inductive generalisations become increasingly confirmed they tend to be used as principles by which the "phenomena" are interpreted. For example, within Newtonian dynamics it would hardly ever occur to a physicist to explain the negative outcome of an astronomical prediction in terms of a failure of the general equation of motion; assuming the latter to be valid, the discrepancy between observation and prediction will "prove" to him that something is wrong with his assumptions concerning the initial and boundary conditions.* Logical positivists tend to characterize the a priori *pragmatically* by just this unwillingness to abandon an a priori principle in the face of an apparently contradictory experience. If two pairs of empirical objects, when counted together, turned out to make five objects, we would not regard the proposition of arithmetic " $2+2=4$ " as refuted, but would rather say that we were wrong in thinking that we had two pairs to begin with, or that the fifth object escaped our attention when we collected the "data", in the worst case we might even invoke a hypothetical demon as the hidden agent who added a fifth object or divided one into two, without our noticing this operation. The irrefutability of logical or mathematical propositions becomes still more obvious, if we consider a fundamental principle like the commutativity of the operation

*A *boundary* condition may be defined as a particular value of a physical constant, varying with the domain of application of the law in which the respective constant occurs as a factor of proportionality. It is to be distinguished from an *initial* condition, which represents a particular value of a state variable at a definite time. The term "boundary condition" is often used generically to refer to any factual condition which, when propositionally formulated, renders a law applicable as an instrument of prediction.

of addition. If, having counted a set of objects a,b,c,d in a certain order, we find that it contains 4 objects, and upon repetition of the process of counting in a *different order*, say, b,c,a,d, obtained a different number, we would suspect that we counted one or several objects twice, or forgot to count one, *etc*, rather than admitting that in some cases the order in which a set is counted affects its cardinality. We want, however, to emphasize that, in respect of this analytic functioning, there is but a difference of degree between mathematical or logical truths on the one hand, and highly warranted inductive generalisations, on the other hand. It is true that the failure of a physical prediction would never be explained by casting suspicion upon the logical and mathematical principles that are implicitly assumed in the deduction of testable consequences. But the same holds true, though to a less degree, with respect to fundamental physical principles such as the general equation of motion or the conservation principles. If, therefore, the validity of logico-mathematical truths is to be *sui generis*, their apriority cannot be defined in terms of their prescriptive function in empirical inquiry.

Let us clarify the meaning of "functional analyticity" in more detailed manner, by reference to procedures of experimental testing. Suppose we are testing a functional law, expressed by an equation involving two variables. If a Cartesian system of rectangular coordinates is employed, the measured values of the function will be graphically represented by ordinates and the measured values of the independent variable by abscissas. One thus obtains a discrete set of points representing "conjunctions" of ordinates and abscissas. To fix our ideas, let us suppose we set out to verify Hooke's law of the proportionality of stress to strain, within the elastic limits of the medium. Stress is defined as force per unit area, and strain, for the special case of elasticity of length, as ratio of elongation to original length. Since the denominators of these ratios (cross-sectional area of the wire, original length of the wire) are constants, the variables to be considered are force and elongation. We shall suppose, therefore, that the ordinates in the Cartesian plane are to be interpreted as values of the force that is experimentally applied to a given wire, while the abscissas are to be interpreted as values of the elongation caused by the application of force (or *vice versa*). If one takes a sufficiently large number of measurements, one will find that the points representing the results of measurement arrange themselves approximately in a straight line. One interpolates by drawing a straight line through the points and interprets this geometrical construction as follows: the law correlating stress and strain is a linear equation, i.e., stress is proportional to strain ("tensio ut vis"), the ratio of stress to strain is the slope of a straight line and hence a constant. This act of

drawing the simplest curve through a discrete set of measured values is predictive: one implicitly predicts that the same "constant conjunction" that has been experimentally verified for some values of the variables that represent empirical properties, will be verified for *all* of their values within a determinate range of variation.

Suppose, now, that, having verified Hooke's law by successive additions of weights to the test wire, we find that upon application of further weights we obtain points deviating from the straight line farther than is justifiable by the assumption of a certain interval of accidental error. In all likelihood, this discrepancy will not be interpreted, by the experimental physicist, as a refutation of Hooke's law, but as an indication that the elastic limit of the wire has been exceeded. Inasmuch as he argues "stress is not proportional to strain, therefore the elastic limit has been exceeded," he is employing Hooke's law as a definitional criterion, since from the falsity of the consequent he infers that the condition which delimits the applicability of Hooke's law is not satisfied. But it would be unduly schematizing the situation to say: therefore Hooke's law is a tautology, irrefutable by experience. At the moment when the physicist explains the discrepancy between measurement and prediction on the basis of Hooke's law, in the way indicated, the latter *functions* as a "universal" proposition, an analytical rule in terms of which one decides whether the material one is experimenting with has exceeded its elastic limit or not. But there are criteria other than Hooke's law for determining whether the elastic limit of the material has been exceeded or not: by definition of "elastic limit," an elastic medium remains within its elastic limit, as long as its deformation is an approximately reversible transformation. Hence Hooke's law remains refutable by experience, even though it may *provisionally* function as "a priori with respect to further operations," in Dewey's phrase.

Analogously, when the physicist experimentally verifies Boyle's law, he may temporarily explain unreasonable discrepancies between results of pressure-volume measurements and predictions implicit in the drawing of a hyperbolic branch through the points representing the pressure-volume values already measured, by assuming that the condition of constant temperature is not satisfied; the non-fulfilment of the predictions, that is, is interpreted as a "systematic error." Boyle's law states: if the temperature is constant, the pressure of an ideal gas is inversely proportional to its volume. If the physicist argues, *tollendo tollens*: the consequent fails to be verified, hence the temperature must have changed, he temporarily uses Boyle's law as an analytical rule, an instrument for locating the "trouble." His inference to temperature

fluctuations, however, is but conjectural: a consultation of the thermometer will resolve the "problematic situation." Again, when two weights suspended at equal distances from the fulcrum of a lever fail to bring about equilibrium, the experimental physicist will conjecture that the suspended weights are not equal; in drawing this tentative inference, he uses Archimedes' law as a "universal" proposition, a "criterion of reality." His inference is "problematic" rather than "apodeictic" because he has other principles at his disposal to test the equality of weights. He may test the equality of weights by means of a spring balance, whose indications are interpreted on the basis of Hooke's law.

Conventionalists who emphasize the possibility of making laws irrefutable by treating them as definitions are prone to overlook the fact that in actual scientific procedure, it is not legitimate to resolve a conflict between law and experience by definition, unless the *apparent* failure of the law can itself be explained in terms of a *systematic error*. Let us illustrate this important feature of scientific method by the law "phosphorus melts at 44° C." Suppose we find a substance which exhibits all the properties of phosphorus except its melting point; by definition, thus one might argue, it is not phosphorus, hence the law remains valid. But science in its advanced stages does not define substances by a haphazard collection of observed properties. If its definitions are to have any predictive value, the *definiens* must hang together as "dependable signs" (in Dewey's words), i.e., they must be causally connected properties. Thus the melting point of a substance depends on the latter's density; hence it is improbable that a substance similar to phosphorus in all respects including the density, should melt at a different temperature. In refusing to classify a substance of the kind described as phosphorus, one thus implicitly declares the failure of a law (correlating the density and the melting point of a solid) that is embodied, so to speak, in the very definition of phosphorus. One asserts the existence of a substance which exhibits some characteristics of phosphorus, and yet fails to exhibit a characteristic which is causally dependent upon those other characteristics. To explain such an anomaly, one may, for example, refer to the law that the melting point of a substance depends on the pressure to which the substance is subjected, and tentatively advance the hypothesis that the substance in question did not melt at 44° because the pressure under which phosphorus melts at 44° was not experimentally realised. This inadequate realisation of experimental conditions is, then, a systematic error* which explains the apparent failure of the law "phosphorus melts at 44°."

* A systematic error, as contrasted with an "accidental" error, is *eliminable* through control of disturbing factors and calculation of their disturbing effects.

Another example, discussed by Poincaré,¹ may make this point still clearer. Suppose astronomers find that a planet does not move exactly in accordance with the law of gravitation. Since so much of the success of celestial mechanics depends on the simplicity of the law of gravitation, it would be very inconvenient to resolve this "problematic situation" by modifying the quantitative expression for the force of gravity.* The nominalist (as Poincaré calls the kind of conventionalist from whom he wants to dissociate himself) will triumphantly point out that this proves that the statement "the force of gravity obeys Newton's inverse square law" merely *defines* what we mean by the "force of gravity" (Actually, of course, "gravity" has, besides its quantitative measure, expressed by Newton's law, a *qualitative* meaning, and if the word be taken in this, so to speak, "pre-analytic" sense, the above statement is synthetic. One might analogously argue that the statement "a freely falling body falls in accordance with the formula $s = \frac{1}{2}gt^2$ " merely defines what is meant by a "freely falling body"; but it is clear that when Galileo enunciated this law, "freely falling body" had, for him, a qualitative meaning, even though after the discovery of the law, the formula $s = \frac{1}{2}gt^2$ may be used as a *criterion* of a free fall). Yet, in order to explain the *appearance* of a failure of the law of gravitation, astronomers will have to invoke the action of another force, as a cause of systematic error. Even if the law of gravitation be construed as a definition, there remains, therefore, the empirical assumption that the force of gravity is the *only* force that acts upon the planets.

To recognize the analytic functions performed by differential laws like the law of gravitation, the *tour de force* of construing them as irrefutable definitions is not required. Thus, if a planet should be observed not to move in accordance with the differential equations that determine its motion, one will suspect that one has not taken all the initial conditions into account; in fact, it has happened, in the history of astronomy, that such a discrepancy led to the discovery of a new planet, initially postulated as a "disturbing factor" responsible for the

*A noteworthy problematic situation of this kind presented itself with respect to the moon's centripetal acceleration, calculated on the basis of the inverse square law. The moon's centripetal acceleration was first calculated in terms of the formula $a = 4\pi^2R/T^2$, where R is the radius of the moon's orbit and T the period of revolution. It turned out to deviate slightly from the value $g/3600$, which value was to be expected on the hypothesis of the inverse square law, since R is about 60 times as great as the radius of the earth. This discrepancy could, however, be amended by abandoning the definition of equal times in terms of equal angular displacements of the earth, considering that, owing to friction, the earth's rotation is not strictly uniform. This incident in the history of physics is epistemologically significant, since it illustrates how, in order to save the simplicity of the laws of mechanics, one may not even shun a revision of the methods of measurement of fundamental magnitudes.

discrepancy. It is a most common procedure in dynamics to use the special laws of motion (mathematically formulated as differential equations of the second order) which render future positions of a body predictable as a function of the time as *criteria* of the completeness of the conditions that determine an initial state. If all the relevant parameters that determine the initial conditions (in Newtonian dynamics, the instantaneous positions and momenta of the component particles of a system) are known, any future position may be predicted by integrating the differential equations. If, however, the predictions should not be verified, one will not declare the differential equations to be inapplicable, but will infer either that not all the relevant state variables have been taken into account, or that the system was not closed. Here again, laws of nature exercise a prescriptive function, even though they first had to be inductively confirmed, and are in principle subject to empirical disconfirmation.

The astronomer's habit to account for a failure of his predictions by finding fault with the instantial propositions that supplied a point of application for his differential equations, and leaving the latter intact, illustrates a general principle of experimental testing. Since no instantial propositions are derivable from a universal major premiss alone, but only from the conjunction of a major premiss with other instantial propositions (functioning as minor premisses), a "negative instance" will never contradict a major premiss by itself, but only a conjunction of a major premiss and a minor premiss, a "theory" and a "fact." Formally, to be sure, the totality of the premisses of a syllogism may be represented by one propositional variable, and then the *tollendo tollens* mode of inference will apparently result in the negation of *one* proposition: p implies q ; q is false; therefore p is false. If, however, we introduce quantifiers and distinguish between universal major premiss and singular minor premiss, the situation becomes more complicated. We may schematize the deduction of an instantial proposition from a synthetic universal proposition (an empirical law) as follows: for every x , if x is S , then x is P ; a is S (where " a " stands for a particular); therefore a is P . To this *ponendo ponens* deduction there corresponds (in the sense of logical equivalence) the *tollendo tollens* deduction: a is not P , therefore, *either* a is not S , *or* it is false that for every x , if x is S , then x is P . It is thus evident that what is implied by the negation of the derived factual consequence is not the falsity of the universal affirmative, but the material incompatibility of the universal affirmative with a supposed "fact" (a is S). If such and such is not the case, then we cannot accept both the law and such and such other fact; either we were wrong in

believing *this* to be the case (individual *a* to be an instance of the kind *S*), or we must reject the law. The so-called "negative instance," which allegedly disproves the law, resolves upon examination into two distinct instantial propositions, one of them affirmative, the other negative. Suppose the law which is subjected to empirical test is the proposition "all crows are black." Then the fact that *this* crow is not black refutes the law. Upon analysis, however, this apparently simple fact resolves into two facts: a) this individual is a crow, b) this individual is not black. If we have reasons to believe that there is a causal connection between the anatomic or physiological constitution of crows and their black color, we may well doubt whether a non-black bird could be a crow; in that case we will not infer from "this individual is not black" the falsity of the generalisation "all crows are black" but will examine more carefully whether this bird "really" is a crow.* To present one more illustration: the instantial proposition "this isolated body traverses unequal spaces in equal times" directly contradicts the law of inertia, according to which all isolated bodies traverse equal spaces in equal times. This one proposition, however, involves two subsumptive judgments: "this body is isolated," and "this body traverses unequal spaces in equal times." This conjunction is certainly incompatible with the law of inertia. But in so far as the law of inertia functions analytically (without being, on that account, *formally* analytic!), it will, far from being invalidated by that compound instantial proposition, invalidate it: if this body does not move uniformly, it is not isolated, it must be acted upon by unbalanced forces.

Essentially the same consideration applies to laws that cannot be formulated as Aristotelian major premisses of subject-predicate form, but are formulated as mathematical equations stating functional relations between properties represented by variables. Here the "facts" which supply the law with a point of application are expressed by judgments of measurement, assigning numerical values to the variables that are functionally related by the equation (initial conditions) as well as to the constants involved (boundary conditions). The equation functions as a rule enabling the calculation of values of a function on the basis of direct measurement of the properties represented by the

*In his recently published *Methodology of the Social Sciences*, Prof. Felix Kaufmann distinguishes between "empirical laws" and "theoretical laws." Empirical laws are said to be refutable by a single negative instance, while theoretical laws, as "rules of procedure," are not *directly* subject to empirical control. They may be—and as a rule will be—abandoned if, as rules of inference, they are not successful in yielding true conclusions, but experience cannot prove their *falsity*. The present analysis suggests that such a dichotomy is artificial, since *any* synthetic universal proposition may be construed as a tool of prediction or a rule of inference (cf. pp. 8–9).

parameters (independent variables) and the constants. Now, if *measurement* of the function yields a value that deviates from the *calculated* value more than is allowed for by the assumption of accidental errors, we are confronted with a "negative instance." But such a negative instance does not directly refute the law expressed by the equation: it refutes the conjunction of the law and the judgments of measurement that supplied the facts whose knowledge is indispensable for making a predictive use of the law. If we conceive of a numerical law as of a rule for calculating values of a function on the basis of direct measurements of parameters and constants, we may say that the so-called negative instance never directly condemns the rule we employed, but just as much casts doubt upon the correctness of our initial measurements; it is a challenge to re-examine our "data."

As was remarked in the *Foreword*, in order to conduct any experimental inquiry involving the use of measuring instruments, some antecedently confirmed laws must be used as rules for interpreting pointer-coincidences. This is another distinctive way in which empirical laws may function as principles in terms of which observational materials are interpreted, without though being formally analytic and irrefutable. Physical laws form, as it were, a network, in such a way that there are always *alternative* laws available for the measurement of physical properties. Hence a law which is presupposed as a principle of interpretation in one experiment, may itself be tested by another experiment, in which other laws function as principles of interpretation. The term "network," here, is intended as a *simile* for a system of functional relations, the "knots" of which are physical quantities that stand simultaneously in functional relationships with other physical quantities. Thus temperature is functionally related to volume, by the law of Gay-Lussac, and also to pressure, by the law of Charles. Since the indications of an ordinary mercury thermometer are interpreted in accordance with the former law (in the special form for *linear* expansion, $l_t = l_0(1 + \alpha t)$, where α is the linear coefficient of thermal expansion), every judgment of measurement which attributes a definite value to the independent variable t (temperature), presupposes the validity of that law. But since temperature changes may as well be measured in terms of correlated pressure changes, on the basis of the law of Charles, it is possible to test, without circularity, the law of Gay-Lussac.

In view of these analytic functions exercised by empirical laws, how is one to answer the dichotomic question whether the laws of nature are descriptive summaries of past observations *or* rules of inference with a primarily predictive function?

. . . General propositions are logical sums and products with an indefinite (perhaps infinite) number of summands or factors. But such sums and products could never be written out, therefore the expression of the general proposition is always an incomplete expression. The completion of the expression involves the specification of all the values which the argument *x* can assume, and this can only be done when these values are explicit and finite. The only tenable interpretation of general propositions, therefore, is that they are finite logical sums and products, i.e., finite and explicit truth-functions of a specified set of elementary propositions. But this renders the view that laws are general propositions untenable, for general propositions are mere descriptive summaries of the past, whereas laws are used to predict the future.²

If this is an adequate statement of the reasons that led certain positivists (such as Schlick) to deny to laws the status of propositions (cf p. 9, footnote) and to interpret them as mere propositional forms, then this positivistic view suffers from a similar fallacy as Mill's view of the syllogism as a *petitio principii*. Mill regarded the syllogism as a *petitio*, because he interpreted universal major premisses as finite logical products of conjunctions of elementary propositions: the truth of such a "finite and explicit truth-function of a specified set of elementary propositions" obviously cannot be known *before* the truth of each elementary constituent is known, hence the knowledge of the truth of the major premiss, thus interpreted, presupposes the knowledge of the truth of the conclusion. Only a very limited number, however, of the general propositions asserted in science are finite logical products or sums of elementary propositions. Such general propositions, being properly *collective* propositions, shorthand records of sets of singular propositions,* antecede, in the development of a science, the process of deductive explanation in terms of propositions of a different type of generality. Thus Kepler's laws of planetary motion could properly be said to be "finite logical products" of singular propositions, and thus to be "completely expressible," since they refer to a finite collection of planets. But it would be absurd to claim that the law of universal gravitation, which is a generalisation proposed as an explanation of Kepler's laws, and which has the form of a general proposition, is a "mere descriptive summary of the past." The truth of the elementary propositions that are derivable from such a universal proposition by the substitution of values for the

*By a "singular" proposition, here, is not meant a proposition that contains no quantifiers, nor a proposition that involves reference to specific dates, but simply a proposition predicating a property of a describable particular, such as the planet Mars.

arguments is, to be sure, a necessary condition for the truth of the universal proposition, and in this *logical* sense presupposed by the latter. In this sense the truth of any singular conclusion of a syllogism is presupposed by the truth of the major premiss of the syllogism; which does not imply that in advancing the major premiss as a *hypothesis*, one must already *know* the truth of the derived consequence. However, unless one dogmatically declares "meaning" and "criterion of validity" to be synonymous, the universal proposition does not, for that reason, *mean* a logical product of "elementary propositions."

The law of universal gravitation does not *assert* the existence of any particles: "*if* A and B are particles, they attract each other with a force directly proportional to the product of their masses and inversely proportional to the square of their distance"; how, then, could it *mean* a logical product of elementary propositions verified in the past? In fact, many of the "protocol sentences" that enter into the evidence for the law of gravitation, are established with the instrumentality of that very law. Since the sizes of the planets are negligible with respect to their mean distances from each other, these bodies may be treated as point-particles and thus the law of gravitation may be applied to predict their motions. But to verify that a planet revolves around the sun in accordance with the gravitational formula, one will have to measure the mass of the sun. It is, however, just the law of gravitation which is the "conceptual tool" by which the sun is "weighed." The solar mass is computed in terms of the dynamical equation: $G m M/r^2 = m \omega^2 r$, where M is the solar mass and m , which cancels out, the mass of a revolving planet. The above equation follows from the assumption that the only effective force in planetary motion is the centripetal force $m v^2/r$ (which assumption in turn involves the idealisation of the planetary motions as uniform *circular* motions). The only unknown in this equation is M , since " r " stands for the mean distance of the revolving planet from the sun, and the angular velocity ω can be computed in terms of the period of the planet's revolution. But, if the law of gravitation is itself presupposed by the elementary propositions that constitute its verification, how could experience ever refute it? It could not, indeed, be refuted if it were the only means of measurement of celestial masses. But if the judgments of measurement of mass, obtained with the help of the law of gravitation, should conflict with judgments of measurement, respecting the same masses, but derived by means of other principles, this "problematic situation" may render the law of gravitation suspect. The stronger, on the other hand, the evidence already gathered in favor of this law, and the greater, accordingly, its

explanatory value, the greater will be the scientist's reluctance to pay, for the resolution of such a problematic situation, the expensive price of revising or abandoning the law of gravitation.

If it is permitted to apply mathematically precise language to a subject which hardly admits of a mathematically precise treatment, we may say, by way of summary, that the degree of apriority of a generalisation is proportional to the degree of its confirmation. If a generalisation is regarded as highly reliable, it is put to the sort of analytic uses that were under discussion in section IV. The transformation of an inductive generalisation into a definition (discussed in section III) is symptomatic of the fact that the scientist's confidence in its validity has reached a high point. Formal analyticity is, so to speak, the maximum degree of apriority; it represents the upper limit of functional analyticity.

PART TWO

APPLICATION OF THE FUNCTIONAL THEORY OF THE A PRIORI TO NEWTONIAN MECHANICS

IT is one of the merits of the Marburg school of Neo-Kantianism to have disclosed the intimate connections between Kant's *Critique*, especially the *Transcendental Analytic*, and Newtonian mechanics. In the light of Neo-Kantian interpretations (Cohen, Natorp, Cassirer), the "transcendental" problem of the possibility of synthetic judgments a priori grew out of Kant's firm belief in the unshakableness of the foundations of Newtonian mechanics. Kant made a distinction between *pure* natural science (also referred to by him as "*Metaphysische Anfangsgründe der Naturwissenschaft*") and *applied* (empirical) natural science. The former was conceived as an exposition and "transcendental deduction" of the first principles of physics, which were assumed, by Kant, to be neither analytic nor empirical, but synthetic a priori. We shall now apply the functional theory of the a priori to such alleged synthetic a priori principles of Newtonian mechanics. These will be shown to be either functionally analytic—at times referred to as "regulative principles," without foregoing, by this denomination, their claim to *descriptive* content—or inductive generalizations that have been transformed, in the process of formalization, into real definitions (factually grounded conventions), or, indeed, imperatives addressed to scientific procedure.

Logical positivists are in the habit of criticizing Kant's doctrine of "synthetic a priori principles of experience" in terms of the epistemological principle that all significant propositions are either analytic or empirical, such that no apodeictic knowledge of reality is possible. In so far as statements are a priori and necessary, so the logical positivists say, they "assert nothing about reality" but are really statements about the use of language (syntactical statements). The present analysis, it should be noted, in no way contradicts this positivistic working hypothesis. It presents, however, a supplementary emphasis, by focusing attention upon existential inquiry as the matrix of those "linguistic"

conventions that give rise to analytic truth, and upon the ways in which synthetic propositions function analytically without definitely assuming the status of "linguistic" conventions. On the whole, we are following in the footsteps of Poincaré's conventionalism, whose spirit is concisely expressed in our thematic quotation (*vide* Foreword).

I. NEWTON'S LAWS OF MOTION

THE PRINCIPLES from which Kant derived, allegedly, his three categories of relation and the corresponding "Grundsätze der Erfahrung," the principle of the permanence of substance, the principle of causality and the principle of reciprocity (*communio* through *commercium*), are Newton's laws of motion: the law of inertia, the law of the proportionality of force to acceleration, and the law of action and reaction. Let us begin with an analysis of the law of inertia. It is, indeed, only for the sake of exposition that Newton's first and second law will be discussed separately. For conceptually the two laws are inseparable. As a matter of fact, the first law may be regarded as a special case, an immediate consequence of the second law: if $F = ma$, as stated by the second law, then, under the action of no kinetic (unbalanced) forces, i.e., the condition $\Sigma F = 0$, it follows that $a = 0$, i.e., the velocity of the body does not change in magnitude or direction. Attempts have been made to formulate a general principle of motion of which the first and second law are but special cases, as by Poincaré: "L'accélération d'un corps ne dépend que de la position de ce corps et des corps voisins et de leur vitesses."¹ Or Wundt, in his early treatise, *Die Prinzipien der mechanischen Naturlehre*, combines, in an analogous way, the first and second law in the so-called "principle of the externality of causes of motion" ("Prinzip der Aeusserlichkeit der Bewegungsursachen"): there are only *external* causes of the change of state of a body. This joint formulation of the first and second law, vague and useless as it is for the purposes of physics, is interesting from the standpoint of the history of science, since it makes explicit the abandonment, implicit in the laws of motion, of the Aristotelian category of teleological causation.

A. THE LAW OF INERTIA

The law of inertia is the prototype of a hypothetical proposition whose antecedent expresses an ideal or contrary-to-fact condition: "If no [unbalanced] external forces act upon a body, it will continue in its state of rest or motion with uniform velocity in a straight line." It is a fact of observational experience that bodies are never isolated from the action of external forces; the universal presence of causes of friction

brings all observable motions sooner or later to cessation. Friction is thus an external influence which diminishes the *magnitude* of a body's velocity. Likewise, gravity is an external influence which changes the *direction* of a body's velocity: thus the straight trajectory which a projectile would have to describe if the antecedent clause of the law of inertia were satisfied, is, by the action of the earth's gravitational field, deflected into a parabola.* It is thus an empirical fact that the necessary conditions for inertial motion are never realized. But Galileo's experiments with the inclined plane revealed inertial motion as an ideal limiting case of accelerated motion. When a body descends on an inclined plane, it will, in virtue of the kinetic energy acquired at the bottom of the plane, ascend to the same height as it has fallen on a symmetrically adjacent plane. The smaller the angle at which the adjacent plane is inclined, the smaller the retardation of the body as it uses its kinetic energy up in doing work against gravity. In the limiting case, when the angle of inclination is reduced to zero, the velocity of the body will remain constant if it were not for the frictional resistance against which work still has to be done (cf. Mach, *Mechanics*, ch. II, section 8). Now, it may be *technically* impossible to eliminate the friction caused by the air (i.e., to produce a vacuum). But the elimination of the friction offered by the horizontal plane itself is impossible in *principle*, since only a weightless body could move without friction, and according to the law of gravitation there are no weightless bodies. Inertial motion is thus empirically impossible,† and this is what exempts the law of inertia from the category of ordinary empirical laws.

Surely, extrapolation from actual cases to an empirically impossible case is not inductive generalization in the ordinary sense, where one simply generalizes from the observed to the *observable*. Thus it seems clear that the principle of Aristotelian physics that motion passes of itself into rest and that the continuance of motion, no matter whether uniform or accelerated, requires persistent efficient causes, has *prima facie* more observational evidence in its support than Galileo's principle of inertia. Thus it is noteworthy that in Galileo's *Discorsi e dimostrazioni intorno a due nuove scienze*, Simplicio, the spokesman of Aristotle-

*In this connection it may be mentioned that Euler defended the necessity of the concept of absolute space, by pointing out that, since according to the law of universal gravitation all material reference frames are accelerated, inertial motion could exist only relatively to a non-material reference frame, i.e., absolute space.

†One may, indeed, imagine a single particle in empty space, infinitely removed from all masses that could disturb its state of rest or uniform velocity. But, according to Mach's kinetic definition of mass, a particle possesses mass only in so far as it interacts with other particles, mass, that is, is a relational property. Hence, if Mach's kinetic definition of mass is accepted, it would be meaningless to endow such an isolated particle with mass, and thereby such a 'thought experiment' is deprived of all physical significance.

lian physics, is a stubborn observationalist. Galileo's ideal forms of motion, he always insists, are all right mathematically, or in the abstract, but there is nothing *in rerum natura* that corresponds to them. The Aristotelian principle "*cessante causa cessat et effectus*," which is an inductive generalization converted into an allegedly self-evident axiom, was, however, menaced by an apparent exception, viz the motion of projectiles. If a projectile is fired into the air it persists in parabolic motion long after the cause of its motion, viz., the initial impulse given to it by the explosion of gunpowder, has "ceased"; apparently, hence, "*cessante causa*," the effect still persists. But, indeed, logically it is always possible to adhere to a seemingly self-evident axiom, and to "save the appearances" in terms of it. Thus the Aristotelian physicists were not embarrassed to account for the motion of projectiles in terms of the axiom "*cessante causa cessat et effectus*": the cause, viz, the initial impulse, really does not "cease," but is transmitted from one "subject" to another, from the gun that fired the projectile to the air and from the air to the projectile, till the latter reaches its "natural place," the surface of the earth. Thus the Ptolemaic hypothesis of geocentricity, too, may be adhered to, provided one does not mind the complexity of the system of epicycles which it makes necessary in order to "save the appearances."

But Galileo saw a simpler way of explaining the parabolic trajectory of projectiles, viz, its geometrical construction, by resolving it into two components, the tangential component representing the inertial tendency to move in a straight line with constant speed, and a vertical component representing the gravitational tendency. The law of inertia, originally established by extrapolation from experiment, thus functions as a rule for the geometrical construction of actual motions. It is a statement about a hypothetical component of actual motions, just as the law of the parallelogram of vectors assumes the causal efficacy of vector components to which no isolated existence can be ascribed. Pairs of vector components are always hypothetical constructions, and since there are always many alternative ways of resolving an actual vector into components, no given component can be said to *exist* physically, unless it can be identified with an approximately isolable physical force, like the force of gravity. In Natorp's well chosen phrase, the concept of inertia, as defined by the law of inertia, is a "Konstruktionsstueck" of mechanics. Or, in the language of Dewey's *Logic* we might say that the law of inertia functions as a "universal proposition." In Kantian language, it is synthetic a priori in the sense of being a "constitutive condition" of mechanics: motion is a possible object of mechanics only in so far as it is geometrically constructible as a curve whose

direction at each point is determined by the tangent (the geometrical representation of the first derivative, which is identical with velocity, if the horizontal axis of the coordinate system represents the time); and the physical meaning of the tangent is just inertial motion.

The law of inertia looks temptingly like a definition, and it has by many writers on mechanics been interpreted as such. There are two possible ways in which it might be regarded as a definition: one might hold that absence of external forces is defined by uniformity of motion. This possibility of regarding accelerated motion as the meaning of "force" we postpone for a separate discussion. The second possibility concerns the meaning of "uniform motion." Uniform motion implies constancy of speed and constancy of direction of velocity. A body moves with constant speed if it traverses equal spaces in equal times. But if by "equal times" we *mean* the times it takes a body isolated from external forces to traverse equal spaces, the law of inertia merely defines isochrony. Actually, this difficulty is avoided by defining isochrony in terms of some periodic process, such as the oscillations of a pendulum. The point of major interest in respect of the logical status of the law of inertia is the meaning of "constant *direction* of velocity." It should be noted, that "meaning," here, has itself an *operational* significance: what is in question is the *method* of ascertaining by measurement that a moving body does not change its direction. If the law of inertia is a *physical* law, the concepts in terms of which it is stated must be *operationally* defined. Now, "direction" is a term that has meaning only relatively to a coordinate system, and if the direction in question is predicated of physical motion, the axes of such a coordinate system, as well as its origin, must be material. But a motion whose direction is constant relatively to an inertial reference frame, will be accelerated if its direction is measured relatively to an accelerated reference frame. According to the law of gravitation, all matter is accelerated on account of being subject to forces of attraction, hence it seems that it is impossible to find material axes in terms of which uniformity could be operationally defined.

This is, as was mentioned already, the argument by which Euler defended the absolute theory of space, and which Russell endorsed in his early critical discussion of Newtonian dynamics: " . . . Any dynamical motion, if it is to obey the laws of motion, must be referred to axes which are not subject to any forces. But, if the law of gravitation be accepted, no *material* axes will satisfy this condition. Hence we shall have to take *spatial* axes, and motions relative to these are of course absolute motions."² In order to reconcile the meaningfulness of the law

of inertia with the relative theory of space, Neumann *postulated* the existence of an "absolutely rigid" body α , relatively to which inertial motion should be defined. But Russell insists that this way of giving a physical meaning to the concept of inertial motion is incompatible with the *universality* of the law of gravitation: "If it is always significant to say that a given motion is uniform there can be no motion by which uniformity is defined. . . . Science holds that no motion occurring in nature is uniform; hence there must be a meaning of uniformity independent of all actual motions—and this definition is, the description of equal absolute distances in equal absolute times."³ "It seems evident that the question whether one body is at rest or in motion must have as good a meaning as the same question concerning any other body, and this seems sufficient to condemn Neumann's suggested escape from absolute motion."⁴ It must certainly be admitted that Neumann's hypothesis of an absolutely rigid body α has no more operational significance than Newton's hypothesis of absolute space;* in so far Russell's criticism is to be approved. It remains to point out the weakness of Russell's argument (to which he himself does not hold any more) for absolute motion.

First, the term "meaning" or "significance" is highly ambiguous. If, for example, isochrony is operationally defined in terms of some natural clock, such as a pendulum, or electromagnetic vibrations of a definite wave-length, it is not claimed that thereby the concept of isochrony has been analyzed. Hence the statement "the period of a pendulum is constant," where isochrony has been *operationally* defined in terms of the period of a pendulum, is meaningful in the sense of being *non-tautological*† Nevertheless, the procedure to *verify* this statement in a system of mechanics in which the pendulum has been adopted as the standard clock, would be circular, and consequently the law of the isochrony of the pendulum is, within that system of mechanics, a convention, not a hypothesis to be tested. The same holds, *mutatis mutandis*, for the operational definition of uniformity in terms of a selected reference frame, say, one whose origin coincides with the center of gravity of our solar system and whose axes are determined by fixed stars. The state-

*One may, indeed, as suggested by Professor Nagel, define absolute space *operationally* and implicitly as any reference frame with respect to which the law of inertia is physically true (cf. p. 18). But such a definition would not be relevant to the *historical* controversy between the defenders of the absolute theory of space and Neumann, who wanted to avoid the postulation of absolute space.

†Analogously, a conditional definition, which states a sufficient casual condition for the occurrence of a certain property, expresses a *law*, and not an identity, the way *explicit* definitions express identities; although the fact that a law is used for purposes of conditional definition indicates that it has been "erected into a principle," as Poincaré would say.

ment that such a reference frame is non-accelerated is not tautological, and accordingly the statement that it is accelerated is not meaningless in the sense of being self-contradictory. But it is meaningless in the sense that, within the system of mechanics in which uniformity of motion has been operationally defined in terms of that reference frame, the accelerated or inertial state of this frame is not a matter of empirical verification. To make the same point in a different context: if temperature is operationally defined in terms of volume changes, the law of thermal expansion is a convention, not an empirical law, as long as we abide by that operational definition of temperature. Nevertheless, it remains, in a non-operational sense of "meaning," meaningful to say that changes of temperature, where "temperature" refers to the familiar secondary quality, cause changes of volume. Operationally, such a causal law (law of correlation) is, in the context of inquiry considered, meaningless; for it has been decided to measure changes of temperature in terms of changes of volume, such that *operationally* a change of temperature *means* a change of volume.

Secondly, we have to criticize Russell's claim that, on the relative theory of space, the universal applicability of the law of gravitation deprives the law of inertia of physical meaning. Any law of nature, after all, is but *approximately* true. For a law of the form "for every x, if x is S, then x is P" to have existential import, the following existential proposition must be approximately true: "there is an x, such that x is S and x is P." With respect to the law of inertia this means that at least one body must be found which is both approximately isolated from gravitational forces (being sufficiently removed from neighboring matter) and whose state of rest or of motion is approximately constant. Obviously, this condition is satisfied by a fixed star, since its great distance from other fixed stars makes the latter's gravitational effects negligibly small. If, on the other hand, the law of inertia is taken as *absolutely* true, then it at once ceases to be a *descriptive* law, and one could not call it "true" in the same sense in which a descriptive law is called "true." The concept of uniformity is, then, in Kantian language, a "regulative idea," in the sense of defining an ideal type of motion, in relation to which the presence of disturbing forces is inferred. In short, in so far as the law of inertia is *absolutely* true, it is not an empirical law, but a *definition* of uniformity in terms of absence of force. But even so, if this definition is to exercise a regulative function in empirical inquiry, the concept of uniformity would, over and above its *conceptual* definition, have to be *operationally* defined, and thus the necessity of "positing" a certain reference frame as inertial cannot be evaded.

The law of inertia is, in fact, neither purely experimental nor purely conventional. The conventional element resides in the operational definition of uniformity; the motion of a certain isolated particle must first be *defined* as uniform relatively to a chosen reference frame. The empirical fact asserted by the law of inertia is that any other isolated particle moves uniformly relatively to the same reference frame. This method of breaking the law of inertia up into an *a priori* and an *a posteriori* component is by Lange referred to as a "principle of particular determination":

Lange takes any one particle left to itself and *defines* its motion as being one that covers equal spaces in equal times relatively to a certain set of axes. This is conventional. The times are defined as equal in which this particular particle covers equal spaces. The *experimental* part of the law is supposed to be that every other particle when left to itself describes relatively to the same axes equal spaces in times defined as equal by the motion of the first particle.⁵

That the state of uniform motion does not depend on the intrinsic properties of bodies such as their masses and various secondary qualities, but only on the state of other bodies, spatially external to them, is an empirical fact, and cannot be deduced by *a priori* reasoning.

It may be asserted quite generally, that if the fundamental laws of nature are analyzed with care, they will be found to be neither purely experimental nor purely conventional, but to contain both experimental and conventional elements. A good example is the first law of thermodynamics: $dU = dQ - dW$, where "U" symbolizes internal energy, "Q" the heat absorbed by a system, "W" the work done by the same system. If we look merely at the mathematical equation, we will discover no more than a *definition* of internal energy in terms of measurable quantities. However, we should supplement the equation by the interpretative text that dU is a perfect differential. This means *physically* that the change of energy undergone by a system upon which work is done or which does work (e.g., a Carnot engine working between given temperature limits) depends only on the initial and the terminal state, not upon the way in which the system passed from the initial to the terminal state. This kind of independence forms the *empirical* content of the first law of thermodynamics. Of course, in assuming the integrability of dU , one generalizes the experimental findings, since there is an infinity of possible ways for a system to pass from the lower to the upper limit of integration, and only a finite number of paths could be experimentally tried out. But this generalization is the kind of generali-

zation that is involved in the formulation of *any* empirical law as a universal proposition, and hence does not require special mention as an *a priori* element.

In what sense, now, is the law of inertia "synthetic *a priori*"? In the *functional* sense of defining, jointly with the second law, from which it is deducible as the special case $F = 0$, a method of analyzing motions. Descriptive laws must be distinguished from regulative laws that tell us how to arrive at descriptive laws: thus the laws of motion are, in Dewey's language, "procedural means" used in the derivation of the law of gravitation from Kepler's laws of planetary motion; they are, hence, from the methodological point of view, of a different type from the descriptive law of gravitation. This may be shown in terms of formal logic. The second law can be formalized as a conjunction of two general statements, of which the first is a real definition and the second an existential statement: " $F = ma$," and "there are forces which are relatively simple functions of distance." The existential component of the second law is a general statement, an existential quantifier occurs in it. The law of gravitation, now, is a *verifier* of this general postulate, in that it indicates a definite force, viz., gravity, which satisfies the condition of being a relatively simple function of distance. Gravitational attraction is a value substitutable for the argument of the function "relatively simple function of distance."*

B. THE SECOND LAW OF MOTION AND THE RELATIVE THEORY OF SPACE

Like the first law, the second law of motion, as stated by Newton, appears like a definition. Newton defines force (in contrast to inertia, "*materiae vis insita*") as follows: "*Vis impressa est actio in corpus exercita, ad mutandum ejus statum vel quiescendi vel movendi uniformiter in directum.*" From this definition of force it follows analytically that in the absence of force a physical system does not change its state: for force is defined as that which changes the state (described in terms of relative position and momentum) of a physical system. The more precise (or quantitative) definition of force is contained in the following statement (*Philosophiae naturalis principia mathematica*, Definitiones, VIII): "*Est igitur vis acceleratrix ad vim motricem ut celeritas ad motum. Oritur enim quantitas motus ex celeritate et ex quantitate materiae, et vis motrix ex vi acceleratrice et ex quantitate ejusdem materiae conjunctim.*" Here quantity of motion (the Cartesian

*The law of gravitation, like any law of force, is a *boundary condition* which renders the general equation of motion applicable to the prediction of states.

measure of force, combated by Leibniz) is defined as $m v$; "*vis motrix*" is defined as $m a$. If we analyze the second law as stated by Newton in terms of these definitions, it is seen to be in part definitional or analytic: "*Mutationem motus proportionalem esse vi motrici impressae, et fieri secundum lineam rectam qua vis illa imprimitur.*" Change of quantity of motion (*mutatio motus*) has to be proportional to the impressed force, simply because it is by definition identical with it: quantity of motion $= m v$; *vis motrix* $= m a$; but $m a = m dv/dt =$ "*mutatio motus.*" What is synthetic and postulational is but the second part of Newton's law, concerning the line of action of force.

Newton's separation between the *definition* of force and the *axiom* stating the (quantitatively) essential property of force, is thus artificial. What Newton introduces as an axiom is, to use Poincaré's famous phrase, merely a "disguised definition," just as it is the case with some of the "axioms" of Euclid. As Mach puts it:⁶

Est ist . . . nur eine ganz unnoetige Tautologie, nachdem die Beschleunigung als Kraftmass festgesetzt ist, noch einmal zu sagen, dass die Bewegungsänderung der Kraft proportional sei. Es waere genuegend gewesen zu sagen, dass die vorausgeschickten Definitionen keine willkuerlichen mathematischen seien, sondern in der Erfahrung gegebenen Eigenschaften der Koerper entsprechen.

The empirical fact which renders Newton's definition physically significant is that force (where the term "force" is to be taken in its pre-scientific meaning of push or pull) manifests itself through changes of momentum. This discovered property of force became then converted into the defining characteristic, the scientific meaning of "force." Thus, according to Kirchhoff, the product of mass times acceleration is the very meaning of "force." According to Planck, on the other hand, "force" means the *cause* of accelerations, something with which we are intuitively acquainted through our experience of muscular effort and strain. The difficulty with this definition of force as the cause of acceleration—which is probably in accord with Newton's own conception of *vis impressa*—is that it does not apply to inertial forces, like centrifugal reaction forces, which according to Newton himself are effects of accelerated motion.

What seems to be implied by Planck's position concerning the meaning of "force," is that force is not a quantity at all, but a *quality* kinesthetically experienced, and that Newton's second law specifies the quantitative *measure* of this quality. According to this interpretation, "force" no more *means* acceleration than "temperature" means length or

"light" means electromagnetic vibrations. This kind of interpretation calls attention to the experiential basis of scientific concepts. It cannot, indeed, be over-emphasized that physics "reduces" secondary or tertiary qualities to primary qualities only in the sense of establishing correlations between experienced qualities and qualities susceptible of precise measurement. On the other hand, it is clear that the concept of force, as applied to celestial mechanics, e g, shares but the name with the kinesthetic experience from which it developed. When the physicist speaks of the centripetal force holding the planets in their elliptic orbits, all the evidence he has for the action of a force is kinematic, viz, change of the direction of tangential velocity; even though it is in virtue of the analogy to circular motions in which the centripetal force is *felt* as a tension, a muscular strain, that the term "force" is applied, there, at all (cf Part One, III, p 24).

Another position that might be taken with respect to the status of Newton's second law is that it amounts to the *kinetic* definition of force, involves, however, the experimental fact that force, *statically* defined by the principle of the lever, *satisfies* this kinetic definition of force. For example, if a weight is suspended on a string, then two static (i e, balanced) forces act on it, viz, the tension in the string and the force of gravity which is equal and opposite to the tension. When the condition of equilibrium is disturbed by, say, cutting the string, the force of gravity is observed to produce an acceleration, and thus to satisfy the kinetic definition of force.

Once acceleration is determined as the measure and scientific meaning of force, there still remains the gap between the *general* equation of motion (Newton's second law) and *specific* laws of force, like the law of gravitation, the law " $F = -kx$ " for simple harmonic motion, the law of electrostatic attraction, etc. To bridge this gap, to render, in other words, the second law applicable as a fruitful definition, the further assumption is required that acceleration can be measured as a relatively simple function of the state variables relative position (distance from neighboring masses) and velocity: "L'accélération d'un corps ne dépend que de la position de ce corps et des corps voisins et de leurs vitesses."*

This assumption again is not analytic, nor is it an ordinary empirical hypothesis. It is a regulative principle, and in this sense "synthetic a priori": in the face of an apparently contradictory instance we resolve to stick to our hypothesis, since the latter is so general that its abandon-

*Notice the qualification "voisin", it signifies that there are such things as dynamically closed systems; an assumption without which a science of dynamics would be altogether impossible.

ment would force us into a troublesome modification of our scientific system:

Si alors l'accélération d'un des corps que nous voyons nous paraît dépendre d'*autre chose* que des positions ou des vitesses des autres corps visibles ou des molécules invisibles dont nous avons été amenés antérieurement à admettre l'existence, rien ne nous empêchera de supposer que cette *autre chose* est la position ou la vitesse d'autres molécules dont nous n'avions pas jusque-là soupçonné la présence.⁸

Just as the law of inertia provides a criterion for the action of forces, thus this postulate provides a criterion for the existence of masses whose relative positions determine forces.

The postulate to explain the change of state of a body in terms of the states of external bodies is closely allied with the *relative* theory of space, and did not seem to Newton to work in all cases. The apparent exception was constituted by the phenomena of absolute rotation. We have seen that if the concepts in terms of which the law of inertia is formulated, in particular the concept of uniform velocity, are operationally defined, it has, like physical laws that are less fundamental, an approximative character. But for Newton himself uniform motion was defined in terms of absolute space and absolute time. These are, however, unobservable, hence, in accordance with the positivistic postulate of observability, the law of inertia, as intended by Newton, seems to be devoid of meaning. Yet, it is often overlooked that Newton's hypothesis of absolute space is not the offspring of metaphysical speculations, but was intended as an explanation of observable physical effects. The following is, roughly, Newton's argument in behalf of the existence of absolute motion, i.e., motion relatively to absolute space: a defender of the relative theory of space must be able to explain all dynamical effects in terms of *relative* motions of bodies. Rotation is a form of motion; hence a relativist must be able to explain the dynamical effects of rotational motions, in particular centrifugal effects, in terms of *relative* rotation. The following experiment seemed, however, to prove to Newton, that centrifugal effects can be explained only as effects of *absolute* rotation, i.e., rotation relatively to absolute space. When a bucket filled with water is set rotating, the water first remains at rest, such that in the beginning of the experiment its velocity of relative rotation (i.e., velocity of rotation relatively to the bucket) is a maximum, while the water is at rest, its surface remains flat. Gradually the water is set rotating by adhesive forces, and as its velocity of rotation (relatively to some fixed reference frame) increases, its surface is depressed in paraboloidal form. When

the water has completely picked up the angular speed of the bucket, such that its velocity of rotation relatively to the bucket is zero, the depression of its surface reaches a maximum. But when the rotation of the bucket is brought to a sudden stop, the water, which continues to rotate with the same velocity of *relative* rotation (the reference body being the bucket) as at the beginning, retains its paraboloidal surface. But if the depression of the water surface were due to the *relative* rotation of water and bucket, how could it be that it exists both when such relative rotation goes on (at the end of the experiment) and when it does not exist (in the middle of the experiment)? *Ergo*, Newton concludes, the centrifugal forces which depress the water surface must be created by the water's *absolute* rotation, i.e., rotation relatively to absolute space.

Newton's belief in absolute space is thus by no means incompatible with his methodological maxim *Hypotheses non fingo*. If the described phenomenon could not be explained in terms of relative motion, absolute space would be as good a physical construct of explanation as any. However, as Mach has shown, we do not need this hypothesis. We merely have to choose a different reference frame in order to solve the contradiction that the same physical effect appears when there exists relative rotation and when relative rotation does not exist. The depression of the water surface cannot be explained in terms of the rotation of the water relatively to the bucket, but it may be explained in terms of its rotation relatively to the fixed stars. To be sure, it sounds unbelievable that the depression of the water surface should be caused by the presence of the fixed stars. But as Broad has emphasized,* all that is required for a defense of the relative theory of space and motion, is the explanation of such rotational effects in terms of rotation relatively to *some* material reference frame; it is not necessary that this reference frame be constituted by the fixed stars. What is essential for our topic is, that the proposition "all acceleration is relative," i.e., $a = f(r)$, where "r" stands for the distance of the accelerated body from other bodies, is itself a regulative principle, and in this sense "synthetic a priori."

That acceleration, and consequently force (being defined in terms of acceleration), is expressible as a function of relative position, is an empirical fact. But it is characteristic of the process of converting empirical laws into "conventions" or a priori principles, upon which we lay, following Poincaré, so much emphasis, that in a post-Newtonian postulational treatment of mechanics this empirical law appears as a formal, conventional postulate. For Newton, the definition of force in terms of acceleration is one thing, and the *fact* that there are *central* forces, i.e.,

*Broad, *Scientific Thought*, pp. 99-113.

forces acting at a distance between particles, such that their direction coincides with the straight line joining the interacting particles, and their magnitude is a function of the distance between the interacting particles only, is quite a different matter. But Boltzmann, in his *Principe der Mechanik*, postulates that the acceleration due to the interaction of two particles depend only on their distance. Since force is, in this postulational treatment, *nominally* introduced as a name for the product of an acceleration by a constant ratio, called "mass," it follows, of course, *by definition*, that force is also a function of distance and shares all the vector properties of acceleration.

C. THE THIRD LAW AND THE INDEPENDENCE OF FORCES

We finally come to Newton's third law. Kant interpreted this law to express universal "reciprocity" or causal interaction, which alone renders objective judgments about spatial coexistence possible. However, the "synthetic a priori" principle presupposed by this law is not so much a principle of causal continuity as the opposite, a principle of abstraction or *closure* of finite physical systems. For the third law is a statement about the behavior of forces that act in pairs; it thus splits the totality of forces up into pairs of forces equal in magnitude, identical in direction and opposite in sense. It defines mass as a constant ratio of opposite accelerations, being always produced in pairs, and it is just the invariance of these ratios with respect to variations in the surrounding field, which is meant by the classical independence of mass from position and velocity: "The ratio of the acceleration of A due to B to the acceleration of B due to A must always be considered to be the same whatever be the position of A and B, and whatever be the surrounding field."⁹ Cournot formulates this principle of closure, without which classical mechanics would be impossible, as follows: ". . . chaque élément d'un système matériel exerce sur un autre élément du même système, ou sur chaque élément d'un autre système, l'action qui lui est propre, absolument comme s'il n'y avait que ces deux éléments en présence, et que tous les autres éléments fussent anéantis."¹⁰

This principle is clearly presupposed by the method of vector addition. For the special case of force vectors, it bears the familiar name of the "principle of the independence of forces": in computing the resultant of several force components, one assumes that each component force produces the kind of motion which is its "proper" effect, as though no other forces were present that might modify this hypothetical effect; or rather, the partial effect of the partial cause is *hypothetical* just in so far

as modifying influences of the other partial causes are disregarded. To be accurate, one should not formulate the principle of the independence of forces in terms of *vector* components: for a vector component is, by definition, a quantity to which the rules of vector addition are applicable, such that, if formulated in terms of "vector components," the principle of the independence of forces is analytic. But the synthetic character of the principle becomes apparent if we express it as the postulate that forces admit of vector addition. It is a "constitutive condition of experience" (which does not preclude its having an experimental basis) in so far as it is implicitly presupposed, whenever a force is mathematically conceptualized as a vector. Within *mathematical* physics, it could hardly be contradicted by experience, since "force" is mathematically defined ($F = m \ddot{x}$) in such a way that "forces," in the preanalytic sense of the term, which do not obey the rules of vector addition, would not be "forces" in the sense in which forces are dealt with in mathematical physics. Poincaré's significant words, "Les principes de la dynamique nous apparaissent d'abord comme des vérités expérimentales; mais nous avons été obligés de nous en servir comme définitions,"¹¹ may be applied to this principle just as safely as to Newton's laws of motion. The principle of the composition of forces was originally no doubt an experimental truth constituting the basis of the science of statics of rigid bodies. It was found that several forces acting upon a point may be replaced by a single force which is their *static equivalent*; that is, if the sense of the resultant force is inverted, the point acted upon will be in equilibrium. But within formalized statics, where forces have the properties of vectors, it follows from the very meaning of "force," that a number of concurrent forces may always be replaced by a single resultant, obtainable by the rules of vector addition.

II. KANT'S "PRINCIPLES OF EXPERIENCE"

A. THE KANT-HUME CONTROVERSY

FOR KANT, the task of "transcendental logic" is to show how concepts can "a priori refer to objects." The logic of induction shows, in Kant's idiom, how concepts refer *a posteriori* to objects, i.e., it is concerned with the conditions of validity of synthetic judgments of fact. Formal logic stands at the opposite extreme: it abstracts from all "matter":

Da die allgemeine Logik von allem Inhalte des Erkenntnisses durch Begriffe, oder von aller Materie des Denkens abstrahiert: so kann sie den Begriff nur in Ruecksicht seiner *Form*, d.h. nur *subjektivisch* erwägen; nicht wie er durch ein Merkmal ein Object bestimmt, sondern nur, wie er auf mehrere Objecte kann bezogen werden. Die allgemeine Logik hat also nicht die *Quelle* der Begriffe zu untersuchen.¹

Since formal logic thus disregards the "objective reference" of concepts, the distinction between "analytic" and "synthetic" is not, according to Kant, discussable within its province. In modern terminology, Kant's contention may be versed as follows: an analytic judgment is, roughly speaking, a judgment whose truth follows from the very meaning of its terms, while the validity of a synthetic judgment cannot be discovered by a mere inspection of the meaning of its terms. But since in formal logic the validity of inferences is determined without reference to the *meanings* of the terms involved in the inference, it cannot be decided whether the premisses from which the conclusion is inferred are analytic or synthetic.

Aristotelian logic, which is the kind of formal logic known to Kant (and, indeed, believed by him to be essentially complete), is a normative theory of syllogistic inference: its aim was to establish canons of valid inference. If propositions were analyzed into quantified subject and predicate, it was not in order to determine whether the predicate attached to the subject in virtue of "matters of fact" or in virtue of "relations of ideas," but simply because the purely *formal* validity of the syllogism depended on the relations of exclusion and inclusion, total or partial, of the classes designated by the subject- and predicate- terms. As a

matter of fact, the confusion between existential and non-existential universal propositions, involved in the recognition of subalternation and conversion by limitation as valid modes of "immediate inference" (cf. p. 11, footnote), could not be avoided by Aristotelian logic, just because, basing itself on a realistic ontology, it did not raise the "transcendental" question: does this concept refer to objects? And if so, does it refer to objects *a priori* or *a posteriori*? Is it a mathematical concept defined by postulates and, through the implicitly definitory postulates, *a priori* "constitutive" of a physical object— as, e.g., the concept of a vector is implicitly defined by the rules governing operations with vectors, and is, through these rules, constitutive of physical vector quantities like velocity, force, etc.,—or is it an empirical concept,* abstracted by comparison from empirical objects?

The question "how can concepts *a priori* refer to objects" is synonymous with the question "how are synthetic judgments *a priori* possible?" The inquiry into the possibility of universal judgments *a posteriori*, i.e., inductive laws, had been conducted by Hume, and ended with an associationist explanation of their psychological possibility, and skepticism as to their evidential justifiability. Kant, now, tries to banish Hume's doubts, by calling attention to a class of judgments which cannot, indeed, be validated by an appeal to actual experience, nor by formal reasoning, but can be shown to be presupposed by *possible* experience and thus by all specific inductive laws. The method of validation of these judgments is neither induction nor formal deduction, but "transcendental deduction": they are shown to be presupposed by the possibility of experience and hence the possibility of empirical science.

Now, it seems idle to engage in a debate as to whether Hume or Kant was right, whether Kant refuted Hume, redeemed science from Hume's skepticism, etc. Hume started out from the assumption that there are just and only two ways of evidential validation (to be distinguished from psycho-genetic explanation) of judgments: formal deduction, in the case of judgments expressing "relations of ideas," and induction, in the case of judgments about "matters of fact." Hence he correctly argued that there is no way of evidentially validating, or "justifying," the inductive principle (also called the "principle of the uniformity of Nature"), since the latter is not analytic and is itself presupposed by all inductive validation. And if all science presupposes this principle,

*For a detailed discussion of the distinction between the Aristotelian "abstractive" concept and the mathematical "functional" concept, spiced with a wealth of illustrative material from the history of science and mathematics, see Cassirer, *Substance and Function* (Chicago-London, 1923, English translation).

the doubts that beset the justifiability* of this principle fatally undermine science itself. Kant agrees with Hume that the principle of uniformity or causality is not analytic and is not synthetic *a posteriori*. But in adding "it is synthetic *a priori*," he has not "refuted" Hume except in so far as Hume implicitly claimed exhaustiveness for his disjunction "either empirical or analytic." To "transcendentally deduce" a principle of induction is to *assume* the validity of the inductive laws of science, and then to reveal the conditions on which this validity depends. Hence, if Kant's "transcendental deduction" were intended to refute Hume's *conclusion* that the inductive laws of science are invalid, or rather cannot be evidentially justified (although our belief in them may be genetically explained in terms of habits of association), being acceptable only on the assumption, to be justified, that the order of phenomena will continue to correspond to the order of ideas, it would amount to a *petitio principii*: one cannot, without begging the question, prove science to be valid by assuming its validity. The only sense in which Kant could be said to have "refuted" Hume is, that by an analysis of Newtonian mechanics he refuted the dogmatic assumption underlying Hume's skepticism: the assumption that all judgments are either analytic or inductive and *tertium non datur*. According to Hume's own principle of empiricism this disjunctive judgment must represent an inductive generalization based on an examination of specific judgments. But then Kant was free to reject this generalization, by pointing to a class of judgments, essential to science, which are neither inductive nor analytic.

Hume's phenomenological analyses are guided by the principle "no idea without corresponding impression." If this principle expresses an inductive generalization, then, obviously, Hume called the inductive principle into question by assuming it, and doubt into the latter's validity would entail doubt into the very premiss which rendered it doubtful; on the other hand, nobody would claim that the principle "no idea without corresponding impression" is analytic: it would be trivial to prove the copy theory of knowledge by *defining* an "idea" as

*It should be remarked, though, that if the principle of induction cannot be justified, and all inductive laws derive their justification from the principle of induction, it does not in the least follow that no inductive law can be "justified." If "justification" is *defined* in terms of inductive inferences, the non-justifiability of the "ultimate major premiss" of all inductive inferences is not a regrettable *factual* impossibility, but simply a consequence of what is meant by "justification", just as the non-demonstrability of the *ponendo ponens* rule follows from the fact that this rule *defines* "demonstration." Who would argue that since the rule which defines "demonstration" cannot itself be demonstrated (in the same sense of "demonstration"), the theorems demonstrated with the help of that non-demonstrable rule "really" have not been demonstrated at all?

an image caused by an impression. Consequently, if Hume's argument is to be freed from the charge of circularity, the principle which characterizes Hume's copy theory of knowledge must be neither analytic nor inductive, but "synthetic a priori" in the functional sense: it is a hypothesis which Hume found confirmed in a large number of instances, and hence converted it into an epistemological leading principle: a hypothesis functions as a leading principle, a "convention" in Poincaré's sense, if, in the face of an apparently negative instance we refuse to recognize our hypothesis as defeated, and instead use it as a prescriptive definition which determines the instance in question not to be an instance *of the kind* to which the hypothesis applies. Thus Hume found the idea of causality, involving the idea of "necessary connexion," not to conform to the requirement that there must correspond an impression to every idea: when two billiard balls hit each other, we do not "see" any "necessary connexion" between the antecedent and the consequent motions. He resolved the conflict by declaring causality, in the sense of necessary existential connection, not to be an idea at all, since it did not satisfy the above requirement. Hume thus founded his empiricism on a non-empiricistic treatment of a certain hypothesis as ultimately and irrevocably prescriptive.

Kant in no way "refuted" Hume, but seized the other, in a sense more "empiricistic," alternative of accepting the *fact* that science operates with concepts that are not in any sense copies of impressions, and constructing an epistemology that would justify the fact of science. If no impressions correspond to concepts such as causality and substance, then so much the worse for Hume's sensationalist principle. The choice between abandoning a principle in the face of apparently contradictory facts and calling the alleged facts, by the use of that principle as a prescriptive definition, mere "appearances" (the way Hume called the alleged ideas of causation and substance "fictions of the imagination"), is purely pragmatic, and the conflict between the alternatives cannot be resolved by logical argument. If one chooses to abide by Hume's principle, one may always say that if science uses concepts to which no impressions correspond, such as the concept of a dimensionless point ("that which has no parts"), determined by the intersection of lines without breadth, or the concept of an exact rate of change (a derivative, like instantaneous acceleration), then so much the worse for science. One may, indeed, show that Kant's epistemology is in better accord with science than Hume's epistemology. But whether this is regarded as proof of the superiority of Kant's epistemology depends on whether the authority of science is or is not accepted.

For Kant it is a fact that there are synthetic a priori judgments in physics, i.e., principles that are not analytic, and are yet assumed to be universally and necessarily valid. Kant's problem is to show *how* such judgments are possible (*that* they are possible follows from their existence, which Kant never questioned), i.e., under what conditions the assumption of their universal validity and irrefutability by experience is justifiable. The demonstration of these conditions is contained in the transcendental esthetic and the transcendental analytic of the *Critique*. No object of experience can fail to conform to the conditions under which awareness of *objects* is alone possible; no actual experience can fail to conform to the conditions of possible experience. The fundamental principles of science cannot be refuted by experience, because they explicate, as it were, the very nature of human intuition and understanding. Thus, according to Kant no objective temporal sequence could fail to be a causal sequence, because it is only through an application of the category of causality, i.e., a causal judgment, that we can experience an objective temporal sequence of events as distinct from a merely *subjective* temporal sequence of acts of apprehension. In the order of apprehension the effect is often prior in time to the cause, and it is only through the intervention of causal judgments that we come to distinguish an objective temporal order of events from a subjective, psychological time sequence of events (the psychological time sequence itself may, of course, be rendered "objective" by causal judgments pertaining to psychology). Yet, the problem how far scientific judgments (in Kant's terminology, "judgments of experience" with "objective validity," as distinct from "judgments of perception" with merely "subjective validity") are determinants of the very content of our perceptions, would seem to pertain to psychology rather than to analytic epistemology; and it is, indeed, the vice of Kant's *Critique* that "transcendental deduction" and psychological, genetic explanation are often mixed up. At any rate, we shall confine our attention to Kant's "transcendental" arguments, neglecting the psychology which, to say the least, does not contribute to the cogency of Kant's arguments.

B. THE CONSERVATION OF SUBSTANCE

The connection between the law of inertia and Kant's first analogy of experience is rather obscure. Kant is fully aware that it is not the judgment "substance is permanent" which requires a "transcendental deduction":

In der Tat ist der Satz, dass die Substanz beharrlich sei, taut-

ologisch. Denn bloss diese Beharrlichkeit ist der Grund warum wir auf die Erscheinung die Kategorie der Substanz anwenden, und man haette beweisen muessen dass in allen Erscheinungen etwas Beharrliches sei, an welchem das Wandelbare nichts als Bestimmung seines Daseins ist.

But the synthetic judgment to be proved, according to Kant, *viz*, that there is something permanent in all phenomenal change, is ambiguous. If phenomena are essentially, or by definition, changing, and the above judgment is a *general* existential proposition (there is an *x*, such that *x* is permanent in phenomena), then it itself is a tautology. For, as Kant points out himself, change is predicable only of the unchanging, hence change without something permanent which changes is logically impossible:

Entstehen und Vergehen sind nicht Veraenderungen desjenigen, was entsteht oder vergeht. Veraenderung ist eine Art zu existieren, welche auf eine andere Art zu existieren ebendesselben Gegenstandes erfolgt. Daher ist alles, was sich veraendert, *bleibend*, und nur sein *Zustand wechselt*.

But the synthetic judgment of which Kant gives a "transcendental deduction" is a particular verifier of the above analytic existential proposition: it asserts that some particular kind of entity, *viz*, the *real* in phenomena, is permanent; where by the "real" is meant that of which we are aware only *intensive*, through the "affection of our sensibility" (presumably matter, prior to its "outer intuition" as *res extensa*). This judgment is no doubt synthetic; yet, whether it be a priori or a posteriori, it surely has no connection with the law of inertia, which conditionally predicates invariance not of matter, but of velocity. And that it is velocity, rather than, e.g., position, which remains, in the absence of kinetic forces, constant, is not a priori demonstrable at all, but an extrapolation from experiment (cf. section I A). What is in a sense a priori, is the passage to the ideal limit, to the contrary-to-fact condition: force = 0, in which extrapolation the principle of continuity is implicitly assumed. By the principle of continuity, here, is meant the assumption that a functional relation which has been observed to hold for some values of the independent variable (in this case the retarding force of friction), will hold for all, even unobservable, values of the independent variable (in this case, the value: friction = 0, is unobservable). By calling judgments derived from empirical data by such extrapolation or idealization "synthetic a priori," one need not imply

any such notions as "thought necessity" or "eidetic insight": that the same functional relation should hold when we pass from observable data to unobservable values, from actual experience to impossible experience, is by no means self-evident. It is quite conceivable that, in mathematical language, the function should exhibit a *singularity* at that unobservable value of its argument. Any extra- or interpolation based on the assumption of continuity is, hence, to be justified by empirical verification, not by a priori reasoning in terms of such metaphysical principles as the law of sufficient reason. But idealisation in terms of continuity may be said to be *functionally* necessary, i.e., without it not a single empirical law could be induced from a discrete set of data.

In consideration of the Newtonian background of Kant's analogies of experience, some interpreters of Kant hold that the first analogy of experience is the transcendental counterpart of the Newtonian principle of the conservation of mass. Since this Newtonian principle is, in relativity theory, invalid for motions whose velocity is comparable in magnitude to the velocity of light *in vacuo*, the identification of Kant's "substance" with the definite physical concept of mass deprives Kant's first analogy of its empirical irrefutability. But Neo-Kantian interpreters of Kant tend to immunize Kant's transcendental principle against any possible disconfirmatory empirical evidence, by treating the term "substance" as a variable, so to speak, for which any value may be substituted provided it designates a conservative quantity: if energy, instead of mass, should be found to have the property of conservation, why then "substance" means "energy" and the principle of the conservation of energy verifies Kant's "Grundsatz"*. Let us, then, examine, in which way the energy principle could be said to be a priori.

The principle of the conservation of energy may be formulated in such a way that it amounts to a definition of "isolated system": an isolated system keeps its energy constant in time. But if we find a variation of the total energy, we will infer that the system was not isolated after all†. The reason why a non-tautological formulation of the energy principle presents considerable difficulties is, that it is not easy to define the general concept of energy without making use of the very principle of conservation. Planck, who in opposition to the conventionalists

*cf. Reichenbach, *Relativitätstheorie und Erkenntnis a priori* (Berlin 1920), p. 75

†Another possible way of saving the conservation principle when confronted with a variation of total energy, would be to *postulate* the existence of a hitherto unknown form of energy which compensates for the apparent variation. Such a postulate would, however, have to be experimentally verified by the discovery of a quantitative equivalence, just the way Joule's discovery verified the hypothesis that heat is but a "form" or "manifestation" of energy.

emphasized the experimental character of the principle, defines the energy of a system in a definite state as the total amount of changes ("Wirkungen"), expressed in units of mechanical work, produced in the environment when the system passes from that state to an arbitrarily fixed "zero state." As he states,² this definition is quite independent of the validity of the conservation principle, since it leaves it an open question whether a value of the energy of a system at a definite time (i.e., in a definite state) is independent of the way in which the system passed from the arbitrarily fixed "zero state" to the state in which it is said to have that definite energy. It is, fortunately for Planck's definition of "energy," an experimental fact that such independence obtains, and that, hence, the cited definition of "energy" enables one to determine the value of the energy of a system in a definite state *uniquely*. Again, it is an experimental fact that changes in the environment of a system, such as heating and cooling, are expressible in units of mechanical work (Joule's discovery), and if it were not for this contingency, the concept of energy, as defined by Planck, would be devoid of operational meaning.

Now, suppose that different methods of bringing a system into a specified state result in different values of the energy of the system in that state. In all likelihood such an experimental result would make the physicist suspect that the system which he experimented upon was not in the *same* state after all. Since energy is not itself a state variable—while it is, like force, a function of the state variables—he could not, indeed, *apodeictically* infer diversity of state from diversity of total energy at different times; unless he should decide not to question, come what will, the experimental law which asserts a functional dependence of the energy upon the state variables. But such an inference will be *tentatively* drawn; in so far the energy principle functions as a conceptual tool for recontrolling the measurements of the variables in terms of which a state is described. Also, the energy principle may function as a criterion for correct choices of boundary conditions. With respect to force, a boundary condition is a special law of force, like the law of gravitation. If initial conditions have been checked, and predictions of future states fail to be verified, then one will suspect that one did not hit upon the "right" law of force. Analogously, whenever the energy principle is applied to a particular kind of system, one has to specify a law expressing the energy of that kind of system as a *definite* function of the state variables; such a law is, like a law of force, a boundary condition with respect to the energy principle. If, then, the above

definition of energy does not, in a given case, lead to a *unique* value of the total energy, suspicions may be cast upon the law which, as a boundary condition, mediated the application of the energy principle to the specific case in question.

C. THE PRINCIPLE OF CAUSALITY

The connection between the second law and Kant's second analogy of experience, the principle of causality, is no more obvious than the connection between the first law of motion and the principle of the permanence of substance. In which way is the category of cause and effect implicit in the law that change of motion is proportional to impressed force? If effects requiring causal explanation are identified with changes of the motion of a system, and causes that do explain such changes with external forces, then, indeed, the second law is equivalent to the principle "the cause is proportional to the effect," "*causa aequat effectum*." But in that case the principle of causality is analytic, in fact an identical proposition. For cause and effect, thus defined, are identical: "force" means $m \cdot dv/dt$, i.e., change of quantity of motion (rate of change of momentum with respect to time). However, Kant formulates the principle of causality as the principle that each state (event) follows upon a preceding state *according to a rule*. It is the term "rule" which indicates in which way Kant's second analogy of experience might be connected with Newton's second law. For Newtonian mechanics the principle of causality means that the forces that determine the motion of a given system are conservative; in other words, that any state, past or future, of the system is uniquely determined by a set of differential equations (equal in number to the degrees of freedom of the system), provided values can be assigned to the constants of integration through measurement of initial conditions. If the motion of a system is to be uniquely predictable in terms of differential equations, the forces acting upon the system must be conservative; for otherwise the space-integral of force, the so-called "force function" or "potential energy," does not exist, such that the principle of the conservation of energy does not apply. Since a differential equation expresses a conservative force as a function of the state variables, causality may be said to obtain in Newtonian mechanics in so far as there *exist* conservative forces of the form $F(r, \dot{r})$. Such existence would then be the *synthetic* assertion of the causality principle, if the latter is formulated as the existential proposition: "there exist differential laws of force, expressing a change of state as a

definite (and fairly simple) function of the state variables, such that there is a one-one correspondence between any two successive states of a system."

However, the principle of causality is operative also in domains of science where no differential equations are employed, and hence it is, from the philosophical point of view, recommendable to formulate it without explicit reference to differential equations "Same cause, same effect" is a popular, and at the same time very vague, formulation of the principle. It may be rendered more precise by the following expansion. "Lorsque les mêmes conditions sont réalisées à deux instants différents, en deux lieux différents de l'espace, les mêmes phénomènes se reproduisent transportés seulement dans l'espace et le temps" ³ That is, provided the same initial conditions of a process are realized, the process will be the same, no matter which be the time and place at which the initial conditions are realized * A corollary from this principle is that *absolute* position in space and time is not itself an initial condition or efficient cause† of a process Sometimes this independence of the character of a process from the space and time of its occurrence is expressed by saying that space and time are no efficient causes, or that space and time are "homogeneous" with respect to the laws of mechanics Now, if the causality principle, in the above formulation, is to be empirically applied and tested, it has to be specified just what is to be understood by the "same conditions" of a process, in particular, one has to select the variables or parameters whose instantaneous values are to constitute the "initial conditions" of a process Once the variables descriptive of a "state" of a system (i.e., the type of initial conditions) have been selected, it is a purely empirical question whether a given system is causal, and whether, hence, the principle of causality applies But the a priori character of the principle of causality resides just in its function as an instrument for the selection of state variables

A historical illustration of this *selective function* of the principle of causality is the transition from Aristotelian to Galilean dynamics For the Aristotelian physicists, the initial conditions consisted solely in the

*In experimentally establishing a law, one will, indeed, repeat the experiment at different times and places But this intentional variation of the spatio temporal locus is not due to a suspicion on the part of the experimenting scientist that the spatio temporal locus itself might be a causal factor relevant to the coexistence of properties he investigates into, its aim is rather the elimination of accidentally concomitant conditions that happen to be realised at a particular place and time

†Concerning the identification of initial condition and efficient cause, I might remark that I interpret Aristotle's distinction between efficient cause and formal cause to correspond to the distinction, well defined in mechanics, between the initial conditions and the law of a process.

initial positions of the constituents of a system. Accordingly motion, no matter whether uniform or accelerated, was conceived as a change of state, and the law of inertia, in its Aristotelian version, implied that a body upon which no forces act is necessarily at rest. But the Copernicans (or Galileans) observed that the terminal position of a moving body depends not only on its initial position, but also on its initial velocity. Thus the form of the trajectory of a projectile is a parabola, no matter which be the initial velocity with which it is fired; but the distance from the initial position at which it will reach the earth is a function of the initial velocity. Velocity was thus added to relative position as a state variable, since a future position of a body is uniquely predictable (in other words, determined) only if its initial position and velocity is known. It is hence clear that the state variables of a system are those quantities a knowledge of whose instantaneous values is indispensable for a unique prediction of the future behavior of the system; which is tantamount to saying that the principle of causality functions as a criterion as to which properties of a system are to be selected as descriptive of its states. This function of the principle of causality as a *definition of state* is clearly expressed in Planck's definition of what is meant by the "state" of a system:

Der Zustand eines materiellen Systems in einem bestimmten Zeitpunkt ist der Inbegriff aller derjenigen Grössen, durch deren augenblicklichen Wert der ganze zeitliche Verlauf des in dem System stattfindenden Prozesses vollständig bestimmt ist."⁴

But if the principle of causality thus amounts to a definition of state, what does it "assert about reality," in which sense is it synthetic? What is synthetic is the assertion that the class which it defines is non-empty, that there *are* quantities which satisfy the definition of a state variable in terms of unique predictability. *A fortiori*, which quantities are state variables is a question of fact. Whether a given property is causally relevant to a given type of process, whether, that is, a given property is a state variable with respect to a given type of process, is a purely empirical question. Thus it is a *fact* that in order to predict a position of a body moving in a gravitational field, it is not necessary to know its temperature or its chemical composition; hence temperature and chemical properties are, in the context of mechanics, no state variables (which, of course, does not preclude their being state variables in the context of other selective disciplines, such as thermodynamics). To repeat: if the concept of a state is defined as by Planck, the statement that the instantaneous state of a system uniquely determines all its past and

future states, is purely analytic. What, however, lends physical significance to Planck's definition of state is, to apply Mill's pronouncement concerning the existential relevance of definitions, "that the latter, along with the meaning of a name, covertly assert a matter of fact," this "matter of fact" being that there *are* state variables in the sense defined.

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Suppose, now, that in order to uniquely predict a future state, one had to know the instantaneous values of a very large number of properties. In that case one could, indeed, still maintain, theoretically, that processes are causal, but the principle of causality would then be a rather empty declaration of faith. This consideration shows that the postulate of causality, in the form of the existential proposition that there are state variables, has to be supplemented by a postulate restricting the number of properties or variables in terms of which a state is described. Otherwise the protasis clause "if the instantaneous values of all the relevant parameters were known," would forever express a contrary-to-fact condition, and determinism would be an idle unverifiable hypothesis. In the language of differential equations, this idea has been expressed by Margenau in the form that Nature could significantly be said to be non-causal, if the differential equations describing the motions of closed systems were found to contain very many variables.* If a closed system were observed to behave non-causally, one could, in theory, always save determinism by either widening the boundaries of the system, i.e., including in the initial conditions the initial conditions of masses external to the original system (which is tantamount to denying that the allegedly non-causal system was *closed*† after all), or maintaining that not all the causally relevant properties, i.e., properties descriptive of a state, were taken into account. But this way of saving determinism would be otiose if it were not a fact that there are approximately closed systems, in the following twofold sense: the contribution to a change of state made by masses at very large distances from the system under investigation is negligibly small‡

*cf. "Meaning and Status of Causality," *Philosophy of Science*, vol. I, no. 2

†The term "closed system" covers two different meanings which, although closely allied, in that in each sense of the term "closed system" the assumption that there are closed systems is presupposed by the possibility of classical mechanics, ought to be distinguished. In one sense, a system is said to be closed if the number of *properties* in terms of which its states are described, is finite and fairly small. In the other sense, a system is said to be closed if the initial conditions of the bodies outside a finite neighborhood of the system are causally irrelevant, i.e., need not be taken into account for a unique prediction of the future of the system. It is useful to distinguish this postulate of *spatial* closure from what might be called a postulate of *parametric* closure.

‡"Un corps matériel donné influe d'autant moins sur les mouvements et transformations d'un autre corps qu'il en est plus éloigné" (Painlevé, *op. cit.* p. 9).

(which is a fact, since the force of gravity is inversely proportional to the square of distance, such that, with respect to planetary motions at least, there is an upper limit of the position vector r , above which the factors determining a change of state practically vanish); there is but a small number of properties that are causally relevant to a future state of a system. In classical mechanics, systems are evidently closed in the latter sense of "closure," since the forces that are responsible for a change of state and are mathematically formulated as second order time derivatives, are functions of only relative position and—in some cases—velocity. If it were not an experimental fact that the force of gravity (the prototype of a conservative "central" force) acts upon bodies irrespectively of their intrinsic properties like temperature, color, electrical resistance, etc., depending only on their distances from the point source of gravitational attraction and their masses, there would be no procedural significance in the statement that Nature, in the Newtonian scheme, is causal or deterministic.

It remains to mention what is perhaps the most important *contingency* which redeems the causality principle from the status of an empty hypothesis that is neither verifiable nor falsifiable. It is a fact that the differential equations of the kind of processes dealt with by classical dynamics are "time free," i. e., that the forces which determine a change of state do not explicitly depend on the time, nor on any explicit functions of the time. In general, the time variable may be eliminated from a differential equation by differentiation. Thus, if one were to describe the planetary motions by means of first order differential equations, i. e., differential equations in which not acceleration, but velocity appears as the "measure" of force (presumably this is the kind of differential equation that would have prevailed in Aristotelian physics if the calculus had been of avail in the middle ages), one would find that these equations contain the time explicitly, since the velocity of a body changes with the time, if forces act upon it. An example of such a first order differential equation is the familiar equation: $dz/dt = gt + c$. If one differentiates such an equation, one obtains a differential equation of the second order, in which the time variable has disappeared ($a = g$).

Now, it is quite conceivable that acceleration itself should be an explicit function of the time. In that case it could still be plausibly maintained that Nature is causal, if a further act of differentiation would definitely eliminate the time variable. The only difference between the deterministic universe of Laplace and Newton, and this hypothetical deterministic universe would lie in the *order* of the differential equations that function as instruments of prediction: the differential equations of

motion would be of the third order, and first order acceleration would itself appear among the state variables. But, as Margenau points out (cf. *op. cit.*), unless causality is made dependent upon the existence of differential equations of a relatively low order, it ceases to have a significant contrary: one may always hang on to the hypothesis that if differentiations be repeated a sufficient number of times, the resulting differential equations will be time free. In very general, non-technical language, we might put the matter thus: in the ordinary, unsophisticated sense of the term "causal," the statement that Nature is causal means, that the laws of Nature do not change in time (this is, roughly, what is meant by saying that the differential equations of physics do not contain the time explicitly) And it is, indeed, a fact that, for example, the exponent of the Coulomb force of electrostatic attraction remains -2 throughout the year and does not change with the seasons. If the statement "Nature is causal" is *descriptive* (what some positivists call a "Wirklichkeitsaussage"), we would say that Nature is non-causal if, say, the exponent of the Coulomb force varied from day to day. But we might find laws of the variation of laws; we might find, phantastic as it sounds, that the exponent of the Coulomb force is an analytic function of the length of the day, such that it is weakest in the winter and strongest in the summer. The possibility of finding laws of the variation of laws, and laws of the variations of laws of the variations of laws, etc., corresponds to the possibility of obtaining time-free differential equations by repeating the process of differentiation. If we do not restrict the term "causal" to laws of a *definite* order, we must be prepared to interpret causality as something not expressible by an *indicative* sentence at all, but only by an *imperative* sentence: the principle thus widely interpreted bids the scientist to search for laws. In so far as the term "science" essentially connotes an activity aiming at the discovery of laws, the principle of causality, interpreted as an imperative, may, indeed, be said to be presupposed by the very possibility of science: science is the successful response to the imperative expressed by the principle of causality. On this point philosophers as widely opposite in their attitude towards Kant's critical idealism as Schlick and Cassirer seem to agree.

Let us, now, return to Kant's version of the principle of causality. "Every event follows upon a preceding event according to a rule." By referring to the technique of differential equations we have tried to clarify just what sort of a rule Kant may have had in his mind. The synthetic a priori principle which, for Kant, is presupposed by Newton's second law which states *which* is the cause of changes of motion, is the

postulate *that* every change of motion has a cause;* just as the law of inertia, which states *which* is the invariant under ideal conditions, presupposes (implies) the postulate *that* there is something invariant. For us, indeed, the statement that change of quantity of motion is proportional to impressed force, is analytic, because the scientific meaning of "force" is, within the present language of physics, just rate of change of momentum, or mass times acceleration. But, as mentioned already, this scientific meaning is itself the product of what for Newton and Kant was an experimental law: before a concept that "refers to an object" can be defined by a certain property, one has to discover the properties of the empirical instances of the concept, and select from them the one that serves best as a defining characteristic. It is hence historically understandable that Newton and Kant distinguished between change of motion and action of forces, such that the statement "change of motion is proportional to impressed force, as effect to cause" was for them synthetic. Relative motion is, for them, distinct from true or absolute motion: the former can be "produced" by a mere change of the frame of reference (cf. the possibility, in relativity physics, of transforming fields of force away by an interchange of reference frames!), while the latter is the effect of the action of real forces:

Causae, quibus motus veri et relativus distinguuntur ab invicem, sunt vires in corpora impressae ad motum generandum. Motus verus nec generatur nec mutatur, nisi per vires in ipsum corpus motum impressas; at motus relativus generari et mutari potest sine viribus impressis in hoc corpus.⁵

The principle of relativity, in other words, applies, for them, only to inertial motions, not to accelerated motions. The judgments "A is in uniform motion relatively to B" and "B is in uniform motion relatively to A" are equivalent as "judgments of experience" (i.e., scientific, objectively valid judgments), although they may differ in meaning if taken as "judgments of perception." But the judgments "A is in accelerated motion relatively to B" and "B is in accelerated motion

*"The knowledge of the causal relation is not, in any instance, attained by reasoning a priori," says Hume, for "the effect is totally different from the cause and consequently can never be discovered in it" (cf. Cassirer, *Das Erkenntnisproblem* . . . , vol. II, pp. 25-27). But, Kant replies, if knowledge of any instance of the causal relation is thus a posteriori, it does not follow that knowledge of the causal relation *as such*, is a posteriori. "Er schloss faelschlich aus der Zufaeligkeit unserer Bestimmung nach dem Gesetze auf die Zufaeligkeit des Gesetzes selbst, und das Herausgehen aus dem Begriffe eines Dinges auf moegliche Erfahrung () verwechselte er mit der Synthesis der Gegenstaende wirklicher Erfahrung, welche freilich jederzeit empirisch ist" (*Kritik der reinen Vernunft*, ed. Adickes, pp. 589-90).

relatively to A" are incompatible, not only as judgments of perception, but also as judgments of experience.* In other words, these judgments are *kinematically* (phoronomically) equivalent, but not *dynamically*, since Newton distinguishes between an apparent change of motion, produced by a mere shifting of the reference frame, and a real change of motion, produced by the action of real forces. Putting this in the fashionable Einsteinian language of "invariance": the laws of Newtonian mechanics are invariant with respect to inertial reference frames, but not with respect to accelerated reference frames. If, for example, a gravitational field is said to exist, relatively to an inertial reference frame, one could not, in Newtonian mechanics, assert the kinematically equivalent proposition that the reference frame is accelerated, and thus "transform the gravitational field away." This is so, because, for Newton, accelerations have *veras causas*, viz., forces, and the explanation of observed accelerations in terms of the action of forces is not tautological, but synthetic.

D. THE PRINCIPLE OF "RECIPROCITY" AND THE DEFINITION OF SIMULTANEITY

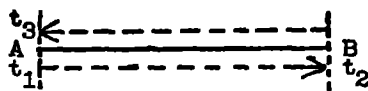
A judgment about an objective time sequence of events is, according to Kant, possible only through a causal judgment: it is the irreversibility of causal sequence that constitutes the irreversibility of time †. But a causal judgment about a succession of states involves, in Newtonian mechanics, both the specification of conservative forces (statement of boundary conditions) and the specification of initial conditions. If, however, the problem of determining the initial conditions of a system at time t_0 is to be meaningful or well defined, the concept of simultaneity has to be defined: to let to time t_0 correspond initial position s_0 , is to assert simultaneity between a pointer coincidence on a clock and a light signal emitted from position s_0 . Thus a judgment of causal succession presupposes a judgment of simultaneity, and this leads us to Kant's third analogy of experience, the principle of *communio* through *commercium*, simultaneous coexistence through causal interaction or "reciprocity." Here again, the psychological side of Kant's argument, viz., the demonstration that the very perception of objective simultaneity would be impossible without the implicit recognition of causal

*cf *Metaphysische Anfangsgründe der Naturwissenschaft*, Phenomenology, theorems 1 and 2

†This intimate connection between time series and causal series is an immediate consequence of the *relative* view of time adopted by Kant, viz., the view of time as an order of events. Thus, as far as in mechanics causal sequences are reversible, i.e., the past can be inferred, logically reconstructed, from the present, time too is said to be reversible—a mode of speech that sounds paradoxical only if we think of *absolute* time.

reciprocity as expressed by Newton's third law, will be omitted from the discussion. What interests us here is the "transcendental" side of the argument: without certain *dynamical* assumptions simultaneity cannot even be defined (physically), hence no judgment of simultaneity could be a "judgment of experience" (i.e., a scientifically warranted judgment). Kant's position is in this respect clearly relativistic, and has been adopted by Einstein himself in his operational definition of simultaneity of spatially distant events. Simultaneity of distant events is defined in terms of immediately apprehended simultaneity of contiguous events: two distant events are simultaneous if the light signals emitted from the loci of their occurrence reach the observer's eyes at the same time.* This definition of distant simultaneity presupposes the dynamical assumption, essential to the special theory of relativity, of the constancy of the velocity of light

Kant's principle of "reciprocity" is relativistic in so far as the dependence of judgments of simultaneity on causal judgments about processes of propagation is recognized: in order to temporally locate a distant event, one must know something about the propagation of sound waves (in the case of an audible event) or light waves (in the case of a visible event), in particular the velocity of propagation. On the other hand, Einstein's definition of simultaneity has refuted Kant's third analogy, in so far as the latter rests on the Newtonian belief in *absolute* simultaneity. Kant accepts the Newtonian principle of *actio in distans*, as involved in the law of gravitation and the assumption of instantaneous propagation of light and gravitational force (the assumption of instantaneous—i.e., infinitely fast—transmission of gravitational force is involved in the application of the third law to central forces), and, in fact, "transcendentally deduces" it: without the assumption of this principle, no judgment of *absolute* simultaneity of distant events would be objectively or physically valid. Einstein seized the other alternative, accepting *actio in contiguum* or causation with finite velocity as a fundamental postulate, and hence rejecting the concept of absolute simultaneity. Einstein defines distant simultaneity in terms of light reflection as follows: Let A = position whence a light signal is emitted, B = observer, t_1 = time of emission, t_3 = time of return



*Since simultaneity will thus vary with the position of the observer relatively to the spatial positions of the events, this operational definition of simultaneity reveals the dependence of judgments of simultaneity upon judgments of spatial location

of light ray to light source. Then, no matter what be the distance AB and the velocity of the light source with respect to the observer, $t_2 = t_1 + \frac{1}{2}(t_3 - t_1)$; the velocity of A (or of B) cannot be taken up into the definition of simultaneity (the way, for example, the coefficient of thermal expansion is taken up into the definition of absolute temperature), because the concept of velocity already presupposes a definition of time equality. *Absolute* simultaneity, now, would mean $t_3 = t_1$, which is, on account of the finite velocity of light, physically impossible.*)

The principle of the constancy of the velocity of light (independence from the velocity of the light source and the velocity of the observer) was originally experimental; at least in the sense in which a hypothesis introduced for the sake of explaining experimental results (in this case the negative results of the Michelson-Morley experiment) is experimental. But in the special theory of relativity it has been converted into a "convention," a postulate "constitutive" of what is meant by a "physical object," in so far as the characteristic properties of a physical object *qua* object of mechanics are defined in terms of time; for the physical definition of time involves, as we saw, the assumption of the constancy of the velocity of light.

E. THE CONVENTIONAL ELEMENT IN THE INTERPRETATION OF "PHENOMENA"

We want to restate that we accept Kant's doctrine of "synthetic a priori" principles only in so far as "synthetic a priori" is predicated of *regulative* principles of science, not in so far as it is predicated of ultimate and unchanging constitutive conditions of experience. It is the characteristic contention of Kant's that experience involves a set of structural conditions which are rooted in the very nature of human understanding and intuition, such that it would be logically impossible for these conditions of experience ever to be contradicted by what experience itself discloses. To solve the paradox that the basic principles of geometry and mechanics are both synthetic and necessary, by showing that they are dictated by the very nature of "reason" which first *prescribes* the general character of experience, was the primary motive that led Kant to his "transcendental esthetic" and "transcendental analytic." However, what Kant did not and could not prove, is that these constitutive conditions have no alternatives. Regulative principles will, indeed, be irrefutable by experience as long as they are used *as* regulative princi-

*cf Reichenbach, *Philosophie der Raum-Zeit Lehre* (Berlin 1928), p. 151.

ples; but experience may suggest the *convenience* of the modification or abandonment of a given regulative principle. Such modification or abandonment is never *logically* compulsory; it can be only *convenient* in terms of given ends of inquiry.

The conventional element that enters into physical interpretations of phenomena may be aptly illustrated in terms of the transition from Newtonian to Einsteinian dynamics, with special reference to the function of geometrical presuppositions in dynamical explanation.

In moving a rod in order to compare its length with the length of a spatially distant rod, one is in effect assuming that the length of the rod does not change during its transportation from one place to another. This invariance of length or, more generally, invariance of shape and size with respect to absolute spatial position is a tacit assumption underlying Euclidean geometry, if the latter is interpreted as a physical geometry whose propositions have the status of empirical hypotheses. It is usually referred to as the "axiom of free mobility," which expresses a necessary condition for the physical applicability of Euclidean geometry. To say that physical space is Euclidean is to say a) that the shape of a body is invariant with respect to spatial position, or that a mere displacement cannot deform a body, and b) that similar figures (in the Euclidean sense of "similar," i.e., figures having identical form but different size) are possible in it. The axiom of free mobility is satisfied by spaces of constant curvature. Euclidean space is a special case of such a space, since its curvature is everywhere zero—in intuitive language, Euclidean space is "flat." Strictly speaking, not only a space of variable curvature (one in which the axiom of free mobility does not hold), but also a space of constant positive or constant negative curvature is non-Euclidean, in so far as Euclid's parallel postulate does not hold for it. However, when the question, which became of acute interest with the advent of the general theory of relativity, is raised, whether physical space is Euclidean or non-Euclidean, what is contemplated is mainly the possibility of variable curvature, not the possibility of a constant curvature different from zero. What will concern us now, is whether this question can be answered by a mere appeal to experience, or whether it is but relatively to conventions that it admits of an answer. The usual attitude of both Kantians and realists in the face of the development of consistent systems of non-Euclidean geometry has been, that the human mind is, indeed, free to construct alternative abstract systems, but that only one of these can *apply* to physical space and be thus, in addition to its formal truth (consistency), materially true. In spite of the world-wide difference of opinion between orthodox trans-

cidental idealism and realism as to the source of the empirical applicability of an abstract system—structure of “mind” versus structure of “reality”—there is agreement as to the *uniqueness* of the abstract system which applies to the empirical world. The following will be mainly an exposition of the views of Poincaré and Reichenbach* on the impossibility of deciding by a *crucial experiment* whether space is Euclidean or non-Euclidean.

Before going into the question of variable curvature, let us examine whether experiments could reveal whether physical space is “flat” (Euclidean) or has a constant curvature different from zero. The usual way of experimentally testing the Newtonian assumption that physical space is Euclidean, is to measure the angle sum of a triangle of large dimensions, such as a light triangle. For if space is Euclidean, the theorem that the angle sum of a triangle is equal to 180° must be physically true, whereas if space is Riemannian or Lobatchefskian, the angle sum must differ from 180° . The reason for choosing a *large* test triangle is that, even if the curvature of space were positive or negative, it would approximately reduce to zero if *small* regions are examined; hence, even if space were non-Euclidean, the angle sum of a small triangle would not appreciably differ from 180° . Suppose, now, that measurements of the interior angles of an astronomical triangle, two of whose sides are constituted by light rays, reveal an angle sum differing from 180° . Is it thereby established that astronomical space is non-Euclidean? By no means, for the difference may be due to an experimental error.

This way of defending the Euclidean nature of physical space is, however, rather trivial, since a proposition of physical geometry, like any physical law, must, if it is to be experimentally tested, include reference to an interval of experimental error. Specifically, if a judgment of equality is to be experimentally testable, it must, on account of the limited sensitivity of any measuring instrument, be translated into a special kind of a judgment of inequality: “ $A = B$ ” must be translated

*Poincaré and Reichenbach agree that the applicability of a geometry is not a factual question. However, while Reichenbach, following Einstein, considers the application of non-Euclidean geometries as practically feasible, Poincaré maintained that physics would never dispense with the Euclidean metric, for reasons of *convenience*—the Euclidean metric is *simpler* than non-Euclidean metrics. Thus, for example, the properties of Euclidean figures are independent of the latter’s absolute sizes, whereas this principle of the “independence of form and magnitude” is not valid in non-Euclidean geometries. On account of this possibility of similar figures in Euclidean space, Euclidean geometry is also simpler from the experimental point of view—in order to test its physical applicability no measurements of absolute magnitudes are required, since the theorems of Euclidean geometry are valid irrespectively of the sizes of figures. Einstein nevertheless dispensed with the Euclidean metric, since in terms of a non-Euclidean metric the laws of mechanics assume a simpler form.

into " $|A-B| < \epsilon$," where ϵ is not *any* small physical magnitude, but a *definite* one. Hence, if the theorems of Euclidean geometry are to be physically verified, they must, as Enriques points out, be converted from judgments of equality into judgments of inequality. Thus the theorem that in any isosceles triangle the base angles are equal, would have to be translated into the following proposition of physical geometry: "Wenn zwei Seiten eines Dreiecks sich um weniger als eine gewisse Laenge ϵ unterscheiden, so unterscheiden sich die beiden gegenueberliegenden Winkel um weniger als eine Groesse τ , die von ϵ nach einem gewissen Gesetze abhaengt."⁶ Analogously, the theorem about the angle sum of a Euclidean triangle would have to be translated into an inequality: $|180^\circ - \alpha - \beta - \gamma| < \epsilon$, where α, β, γ stand for the interior angles of the triangle, and the value of ϵ will depend on the accuracy of the measuring techniques. Suppose, now, the experiment reveals that $|180^\circ - \alpha - \beta - \gamma| > \epsilon$. Does this inequality uniquely decide in favor of non-Euclidean geometries? We cannot, indeed, reconcile this result with the truth of Euclid's theorem by a recourse to *accidental* errors. But we may still explain the discrepancy in terms of a *systematic* error. The light rays which formed two sides of the triangle, we may say, were, contrary to our tacit supposition, not "free," but deflected by gravitational forces, hence they did not describe straight (in the *Euclidean* sense of "straight") paths. But, of course, one can then empirically investigate whether the light triangle experimented upon is situated in a gravitational field and whether the intensity of that field renders the hypothesis of a deflection of the light rays plausible. If no such gravitational field can be detected, it seems that the material truth of non-Euclidean geometry is definitely established by the crucial experiment. Yet, there remains to be mentioned a last possibility of "saving" Euclidean geometry: we may reject the law of optics that light travels in straight lines, and accordingly change our material ("coordinating") definition of "Euclidean straight line"*. It is thus obvious that the physical truth of Euclidean geometry is not decidable by a crucial experiment, except relatively to the *convention* not to question certain physical principles, such as the mentioned law about the propagation of light.

*A "coordinating" definition (a term brought into vogue mainly by Reichenbach) of an implicitly defined "primitive" term is by no means arbitrary. For the empirical objects which are "coordinated" with the primitive concepts of an uninterpreted system must satisfy the axioms and theorems of the latter. The formal axioms are propositional functions and the primitive concepts are real variables, the coordinating definitions amount to a set of substitutions of descriptive terms for the variables, which convert the axioms into empirically verified hypotheses.

Let us, now, turn to a consideration of the possibility, endowed with great significance by the general theory of relativity, that physical space might have a variable curvature, such as to satisfy neither the principle of the independence of form and magnitude nor the axiom of free mobility. If physical space has a constant curvature, then, in the absence of deforming external forces, a body must preserve its shape as it moves through space (this conditional may, as a matter of fact, be proposed as an operational definition of "constant curvature"). If the consequent of this conditional is to be empirically verified, it must be possible to isolate deforming external forces. Isolable deforming forces are what Reichenbach calls *differential* forces. They can be "screened off," or at least they have *different* effects on different kinds of bodies. For example, consider the case that bodies move through a thermal field: even if no perfect heat insulators, i.e., bodies whose thermal conductivities are equal to zero, are available, such that bodies could be entirely "screened off" from a thermal field, the thermal field would have different effects on bodies of different capacities of thermal expansion, hence one could extrapolate and infer that in the absence of such a thermal field bodies in motion would not be deformed.

But suppose there exist forces that are not isolable because they act *indifferently* on all kinds of bodies, regardless of intrinsic properties like thermal or electrical conductivity; such forces, which Reichenbach calls *universal* forces, could not be eliminated by experiment, but only by definition. Reichenbach's definition of a universal force is not empty, since the force of gravity is a force which satisfies that definition: as Galileo established by his famous experiment, the acceleration of gravity does not vary with the kind of "material" to which it is imparted. Now, if all bodies are subject to gravitational force, and gravitational forces are not experimentally isolable, how could one decide by experiment whether an observed deformation or acceleration is due to the action of gravitational forces or to a property of space? Reichenbach's point is that the existence of universal forces cannot be established by experiment, but only by definition. If universal forces are excluded by definition, then a deformation undergone by a rigid body* as it moves through space, is empirical evidence for a variation of space curvature. But there is the alternative possibility of introducing into dynamics *by definition*

*A "rigid" body, here, is defined as a solid body upon which no external differential forces are acting. Since this condition of absence of external differential forces is never actually realized, one might be tempted to say that no bodies are rigid in Reichenbach's sense. However, all that is meant by saying that no external differential forces act upon a given solid body is that such forces are negligibly small in comparison with the internal differential forces (forces of cohesion) of that solid body (cf. C. B. Weinberg, "Rigidity, Force, and Physical Geometry," *Philosophy of Science*, 1941).

universal forces, and then the axiom of free mobility (which is equivalent to the assumption of constant space curvature) could be saved whatever deformations or accelerations bodies might be observed to undergo. Since either mode of explaining deviations from uniformity of shape or uniformity of motion has exactly the same empirical consequences, the question whether space has a variable curvature in regions where deviations from uniformity are observed, or whether such deviations are due to gravitational fields in a space of constant curvature, cannot be decided by an appeal to *fact* but only by *convention*. Thus only pragmatic reasons of convenience could induce Einstein to explain the deflection of light rays from their straight paths in intense gravitational fields in terms of the *geometry* of space in such regions instead of *dynamically* in terms of the action of gravitational forces. Nevertheless, what renders the geometrical kind of explanation possible is the *empirical fact* that gravitational forces are non-isolable (on account of their non-differential effects); for on account of this non-isolability it is impossible to verify a causal explanation of non-uniformity in terms of gravitational force, by eliminating the hypothetical cause and seeing whether the effect still appears.

The significance of this kind of analysis is that it reveals a sort of conjugate relationship between metrical geometry and dynamics. Since the evidence for the existence of gravitational fields of force is purely kinematic,* and kinematic effects can be measured only relatively to a given metric, the existence of such fields altogether depends on our coordinating definition ("Zuordnungsdefinition")† of congruence. If we find the results of measurements of space to be discrepant with our initial assumptions as to the metric of space, we may explain such a discrepancy in terms of universal forces: "Sagen wir: es herrscht eigentlich eine Geometrie G , aber wir messen eine Geometrie G^1 , so ist damit zugleich eine Kraft K definiert, welche den Unterschied zwischen G und G^1 bewirkt."‡ This possibility of reconciling the results of measurements of space with a hypothesis as to the "natural" geometry of space by a suitable introduction of universal forces, is what Reichenbach calls the "principle of relativity of geometry":

*A *kinematic* explanation is a mechanical explanation in which no use is made of the *dynamic* concepts of mass and force

†By a "coördinating definition of congruence" is meant a logically arbitrary choice of a rigid standard rod. The length of such a standard rod is by definition invariant with respect to displacements of the rod. Even though such a standard rod must have certain properties (such as a small coefficient of thermal expansion) in order to be eligible as a standard, the question whether it is "really" rigid is meaningless in the sense of admitting of no answer within the system of mechanics in which deviations from rigidity are measured by comparison with this standard.

Sei irgend eine Geometrie G^1 gegeben, welche die Messkoerper befolgen; dann koennen wir immer eine universelle Kraft K so wirksam denken, dass die Geometrie eigentlich die Form einer beliebig zu wachlenden Geometrie G hat und die Abweichung von G auf einer universellen Deformation der Messkoerper beruht.⁸

If, however, one decides to admit only differential forces in dynamics, it will not be possible to maintain the validity of a preconceived type of metrical geometry when actual measurements reveal that physical space exhibits a different kind of metric. That is, *relatively to the convention* to admit no universal forces into dynamics, it is an *empirical* question whether the curvature of space is constant or variable.

It may be suggested, though, that if people disagree as to whether the axiom of free mobility is a necessary presupposition of physical geometry, the ground of the disagreement is that they use the extremely flexible word "space" in different senses. In the unsophisticated pre-Einsteinian sense of the word "space," space is distinguished from the material medium which is distributed in it, and in terms of whose distribution, i.e., relative positions of masses, dynamical effects such as accelerations are causally explained. As Broad points out, a man who maintains that "space," in this good old Newtonian sense of the term, obeys the axiom of free mobility, is really saying something analytic of what he means by "space." For the axiom of free mobility "really amounts to saying that *mere* difference in position makes no difference to the shape or size of a body. Now this denial of causal action is the only way in which space could be distinguished from a material medium distributed throughout space."⁹ The axiom of free mobility thus functions, very much like the law of inertia, as a *standard* with reference to which the presence of disturbing factors, i.e., forces, is inferred. No experience could refute the axiom of free mobility, for understanding by "space" something causally inert, we would always explain a deformation of a moving body in terms of forces, even if we should have to *invent* forces for this purpose. According to the general theory of relativity, however, space has a variable curvature in gravitational fields, the degree of curvature being determined by the distribution of the gravitational potential; consequently the axiom of free mobility is said to be invalid for motions in such fields. But it seems obvious that the *physical* meaning of the mathematical term "varying curvature" is just "non-uniform distribution of gravitational potential." Hence, if one explains deformations of moving bodies in terms of properties of space, one is giving a causal explanation that differs but verbally from

the old-fashioned explanations of such metrical changes in terms of forces, and is obviously using the word "space" in an altogether different sense from the sense in which people who defend the axiom of free mobility use it.

If we examine the way Russell, in his early *Foundations of Geometry*, defended the a priori character of the axiom of free mobility as a necessary presupposition of *any* kind of metrical geometry, it becomes clear that this axiom merely *defines* "absolute position" or "Newtonian space," and that consequently it is abandoned in Einsteinian physics only in so far as the very concept of absolute position is rejected as operationally meaningless. The axiom of free mobility, according to Russell's statement, amounts to the "a priori law that *mere* motion, apart from the action of other matter, cannot effect a change of shape. For without this law, the effect of other matter would not be discoverable"¹⁰ The very reason by which Russell supports his claim that free mobility is an "a priori law," indicates the *analytic* function of the axiom of free mobility: the only empirical criterion for "mere motion," or mere change of *absolute* position, is just the absence of deformation. In other words, the statement that a body changes its *relative* position is equivalent to the statement that "other matter" acts upon it, in the sense that these statements necessarily have the same truth-value. The only criterion that distinguishes a change of absolute position from a change of relative position is the preservation of shape and size, hence the proposition that "mere change of absolute position" is accompanied by preservation of shape and size, is analytic.

The only sense, hence, in which the axiom of free mobility "does not hold" in relativity physics is, that the very concept which this axiom defines, viz., the concept of absolute position, is dispensed with, as being operationally meaningless. Since motion, according to the language of relativity physics, is motion in a "metrical field" with varying curvature, and the *physical* meaning of "varying curvature" is non-uniform distribution or density of matter, "mere motion," in Russell's sense, is meaningless in relativity physics. As a matter of fact, Russell's "principle of relativity," viz., the principle that absolute position has no causal efficacy, could not possibly serve as a criterion for determining whether the geometry of physical space is Euclidean or non-Euclidean. For, as long as we deal with *finite* systems, a change of *absolute* position is always at the same time a change of *relative* position. But the only way to empirically determine whether a given change of state is due to a change of absolute position would be to make a body change its absolute position and at the same time keep its relative position unchanged; the

elimination of all surrounding matter being impossible, a change of shape could always be explained in terms of a change of *relative* position. And if the system considered is the entire universe, it becomes altogether meaningless to speak any further of position and motion in space.¹¹

A general epistemological conclusion to be drawn from this examination of "necessary" presuppositions of mechanics is, that any given interpretation of appearances is never logically necessary, but only functionally necessary, i.e., more convenient in terms of given ends. "Synthetic a priori" principles, therefore, define "reality" never definitively, but provisionally only, and it is only as such *provisional* definitions of the subject-matter of scientific inquiry that they are acceptable in the light of the developments of modern science.

III. IDEALISATION IN PHYSICS

THE TOPIC that will be under discussion in this section is the process of explaining the behavior of complex physical systems by resolving the actual system ideally into less complex systems, and establishing laws that, without further correction, apply only to ideal abstractions. We have emphasized that propositions of the same *grammatical* if-then form may differ in their *logical* form. Thus the propositions "if x is a square, then x is rectangular" and "if x is a crow, then x is black" have the same grammatical form, but differ in logical form in so far as the former is analytic and the latter is synthetic. We shall now focus attention upon if-then propositions that are neither analytic, nor ordinary inductive generalisations that are verifiable by observing the coexistence of the attributes that are asserted to be invariably connected. The kind of propositions referred to are contrary-to-fact conditionals that apply only to *ideal* cases and have essentially instrumental status*. A classical example of such a proposition, already discussed at length, is the law of inertia, which tells us what *would* happen, *if* a body were entirely isolated. If one were to apply Russell's definition of material implication to contrary-to-fact conditionals, it would follow from the very meaning of "contrary-to-fact conditional" that such a conditional is always true: for a material implication is true if its antecedent is false, but the antecedent of a contrary-to-fact conditional is, by definition, factually false. However, in contradistinction to laws like "for every x , if x is a crow, then x is black," which are properly verified by looking for instances that exhibit both the *implicans* and the *implicate*, laws of this kind are verified in terms of deductive consequences† Hence the

*If I call such laws "instrumental," it is not in the sense in which rules of inference are instrumental to the drawing of conclusions, but rather in the sense in which not directly testable premisses are instrumental to directly testable conclusions. I cannot share Prof. Felix Kaufmann's opinion, that such laws are "rules of procedure." "Such laws are not descriptions of an ideal cosmos, but prescriptions for scientific procedure concerned with the actual cosmos. In other words, they are not synthetic propositions, but rules of procedure. We shall call them *theoretical laws* as contrasted with *empirical laws*, which are synthetic universal propositions accepted in science" (1). It seems to me that it suffices to apply Kaufmann's view to specific cases to reduce it *ad absurdum*. All the laws of Newtonian mechanics, for example, apply to frictionless and strictly reversible processes, and it is known that no such processes exist in the "actual cosmos." Hence Newtonian mechanics is made up of nothing but rules of procedure?

†In strict logic, of course, a formal implication of the above sort is also verified in terms of deductive consequences: supplementing the universal proposition with an instantial minor premiss, one deduces a testable instantial proposition. But in the case of contrary-to-fact conditionals, the law to be tested has to be supplemented by other laws, if one desires to derive observationally testable consequences.

freedom of scientific construction is greatly hampered, if the postulate of observability is addressed to this category of laws which are not, without systematic "correction," descriptive of "observables."

The masterly employment of the "resolutive-compositive" method by Galileo constituted the dawn of mathematical physics. In so far as the properties of empirical objects are measurable, such as the pressure, volume and temperature of gases, they can be symbolized by mathematical variables, and the task of finding a law involving these properties will then reduce to the task of discovering a functional relationship between the variables. The resolutive method consists in breaking such a complex pattern of functional relatedness up into simple, preferably dyadic, functional relations, by either assuming a number of variables to remain constant, or assuming them non-existent in the sense of making but negligible contributions to the "behavior" of the function. In contradistinction to classificatory laws, to be found in non-mathematical sciences in their "natural history stage," laws obtained by the resolutive method are contrary-to-fact conditionals since they apply to *ideal* situations. Thus Galileo argued, if a jet were not subject to gravitational force, it would not describe a parabolic trajectory, but would follow an inertial path, shooting off along the tangent of the parabola, and if the frictional air resistance were eliminated too, this inertial motion would continue for ever through infinite space. Such laws represent extrapolations *from* physical experiment and observation, to be sure, but they are extrapolations *to* an unobservable and experimentally unrealisable case, and thus essentially involve what Mach called a "thought experiment."

Laws that involve "thought experiments" and are stated in terms of contrary-to-fact conditions are not *descriptive* in the sense in which ordinary generalisations like "all crows are black" are descriptive. On the other hand, it would be absurd to deny them "existential reference." If the physicist formulates a law in terms of the concept of "isolated system," he is not asserting an inductive generalisation of the kind that are common in classificatory sciences: he knows that there are no strictly isolated systems short of the entire universe (and, indeed, as applied to the universe as a whole, the notion of an isolated system becomes meaningless: what could the universe be isolated *from*?) But if one denies to these laws existential reference on account of the contrary-to-fact character of the conditions expressed in their antecedents, one is misguided by an untenable empiricism, an empiricism that postulates *immediate* correspondence to fact for the propositions of physics. The correspondence to fact, the "existential reference," of these laws is, however, *indirect*: they are hypotheses which, when supplemented by

other hypotheses which, *in isolation*, are all of them equally "abstract," yield deductive consequences that do correspond to fact. Any differential equation, by itself, describes an ideal motion, known to be non-existent, since it describes the kind of motion which a system *would* undergo if it possessed just one degree of freedom (e.g., if it consisted of one particle free to move only along the x-axis, as in the case of the simple harmonic oscillator); but the totality of these differential equations, whose number is equal to the number of degrees of freedom of the system concerned, constitutes a set of hypotheses in terms of which the *actual* motions of the system are predictable, provided initial conditions are otherwise known.

The method of arriving at general formulae by first considering special cases and then reconstructing the complex real case by varying the parameters that were assumed constant, or taking account of the parameters that were intentionally neglected, involves a priori principles such as the principle of continuity and the principle of the isolability of determinative factors. The former principle postulates to generalize a special law by varying the conditions for which that law holds, such that the generalization thus obtained reduces to the special law if the original conditions are reinstituted. Thus, if one assumes the friction on an inclined plane to be zero, the formula for the resultant acceleration is: $a = g \sin \theta$, where g is the acceleration of gravity and θ the angle of inclination. If we include the frictional resistance in the set of parameters that determine the motion down the inclined plane, we obtain the general formula $a = g \sin \theta - g \cos \theta \kappa$. Or, we may consider $a = g \sin \theta$ as a general formula from which inertial motion and purely gravitational motion (free vertical fall) are deducible as special cases by varying the parameter θ . if $\theta = 0^\circ$, $\sin \theta = 0$, hence we obtain $a = 0$ for frictionless motion on a horizontal plane; if $\theta = 90^\circ$, $\sin \theta = 1$, hence the acceleration down the plane reduces to the acceleration of gravity.*

*The very same principle of continuity, which is here illustrated in terms of an elementary example, was operative in the transition from Galilean mechanics to the special theory of relativity. According to the Galilean postulate of relativity, the laws of mechanics are invariant with respect to the group of transformations defined by the following equations $x' = x - vt$, $y' = y$, $z' = z$, $t' = t$, where v is the velocity of a reference frame that moves with constant speed along the x-axis of the original reference frame. That is, all the reference frames that move uniformly relatively to each other (including the special case of rest) are equivalent for the description of mechanical phenomena. It was found by experiment that if the Galilean equations of transformation are retained without modification, electro-magnetic and optical processes do not satisfy the postulate of relativity. The Lorentzian transformation equations, which render the postulate of relativity satisfied by electro-magnetic processes as well, involve the velocity of light *in vacuo* (c) in such a way, that they reduce to the Galilean equations for velocities that are small relatively to c .

A concept that proved to be of great heuristic value in the development of physics (beginning with Galileo), and which is essentially involved in the concepts of continuity and differential quotient, is the concept of *limit**. This concept was utterly condemned by the sensationalists Hume and Berkeley, on the ground that nothing in our sense experience, or nothing intuitively representable, corresponds to it (cf. Berkeley's attacks on Newton's invention of the calculus, in "The Analyst"). What they altogether failed to understand is the instrumental value of concepts to which no "impressions" may correspond, as tools of discovery. Galileo, even though he did not have at his disposal a mathematical theory of limits, which was later worked out independently by Leibniz and Newton, was led to many of his discoveries by a more or less instinctive employment of just this conceptual tool, the concept of limit. As Mach develops in his *Mechanics*, Galileo discovered the law of the simple pendulum, by reducing the motion of a pendulum, not in actual experiment performed with physical tools, but in "thought experiment" performed with the help of the mentioned conceptual tool, to a series of inclined plane motions. For motion on an inclined plane, the law of force is: $F = m g (\sin \theta - k \cos \theta)$, where $m g$ is the weight of the body that is urged down the plane by the force F , and θ the angle of inclination. Since $k m g \cos \theta$ represents the frictional resistance of the plane (k = coefficient of friction, $m g \cos \theta$ = pressure of the body, directed normally towards the plane), for the idealized frictionless case, the unbalanced force urging a body down an inclined plane is given simply by $m g \sin \theta$. Now, if we imagine the continuous oscillations of the bob of a pendulum along a circular arc broken up into small steps along straight line segments (chords subtending the arc), continuously changing in direction, we can "integrate" the oscillatory motion out of infinitesimal motions on infinitesimal inclined planes (where the assumption of frictionless planes corresponds to the neglect of the frictional air resistance). The effective force driving the bob towards its equilibrium position has, then, the same form as the effective force on a frictionless inclined plane: $F = m g \sin \theta$. It can be shown that, for the motion of a simple pendulum, $\sin \theta = x/l$ (where x is the displacement of the bob, and l the length of the pendulum). Hence the simple harmonic motion of a pendulum (whose law is $F = -kx$) is a special case of inclined plane motion; remembering that the validity of this deduction altogether depends on the assumption, which

* 1 is said to be a limit of the sequence of real numbers $a_1, a_2 \dots a_n$, if there is a term a_N , in that sequence, such that for every $a_n > a_N$, $|1 - a_n| < \epsilon$, where ϵ is any positive real number, however small.

later came to constitute the geometrical or intuitive foundation of the integral calculus, that a segment of a circular arc coincides, in the *limiting case*, with the chord subtended by it. Since this coincidence holds only for *infinitesimal* displacements of the bob, the formula for the period $T = 2\pi \sqrt{l/g}$ holds only for small amplitudes of vibration.* The generalised formula for the period of a simple pendulum involves an infinite power series in such a way that it reduces to the special formula for small amplitudes, since for small amplitudes the high power terms of the series are negligibly small

If the methodological principle of continuity, illustrated in the foregoing, is to have physical application, approximate isolations of determinative factors or parameters must be feasible. As a matter of fact, it is experimentally possible to study, for example, the effect of friction by itself, by experimenting on a horizontal plane and thus eliminating the force of gravity † Or, in studying the thermal behavior of gases, one can experimentally keep the temperature constant, when one verifies Boyle's law, or keep the pressure constant, in verifying the law of Gay-Lussac, or keep the volume constant, in verifying the law of Charles.‡

*It might be added that this formula itself is descriptive of an ideal limiting case: the *simple pendulum* is a physical system that possesses only one degree of freedom, i.e., it consists of one particle free to move only along the x-axis. Its motion is determined by one single differential equation ($m \ddot{x} = -kx$), but such a motion is an ideal abstraction. The bar or string on which a simple pendulum is suspended is, in Galileo's treatment, considered *kinematically*, not dynamically: that is, it is supposed to have no mass, and hence not to be subject to gravitational torques, but is taken account of only in so far as it constitutes a "constraint" limiting the possible directions which the motion of the bob may take (the bob, idealized as a particle, cannot, owing to the "constraint" it is subjected to, move in the direction in which the force of gravity acts, the force of gravity, in the terminology of Gauss, is a "constraint" force, not an "effective" force). Once, however, the bar or string by which the bob is connected with the axis of oscillation, is *dynamically* viewed as a linear arrangement of mass particles, the case is more complex in that *rotational* quantities have to be considered. The component of the force of gravity which drives the bob towards the equilibrium position produces torques (moments of force) with respect to the mass particles that constitute the rigid connection with the axis, and accordingly the period of the *real* pendulum, which is a rigid body, not a particle subject only to *linear* motion, will be a function of the pendulum's moment of inertia. This generalisation from the simple or ideal pendulum to the compound or real pendulum was achieved by Huyghens (cf. Mach, *Mechanics*, (English translation) pp. 173-177). It illustrates the method of simplifying a dynamical problem by first taking account only of linear quantities, through the idealisation of rigid bodies as particles, and then approximating to the real case by including in the set of determinative factors of the motion torques.

‡In pushing a body along a horizontal plane, one performs work against the body's *inertia*, not against the force of gravity, if g nevertheless appears in the formula for the unbalanced force $-m a = -k m g -$, it is on account of the proportionality of inertia to weight.

†The critical reader may well wonder why a law that may be experimentally verified by approximately realising the conditions to which it refers should be a "contrary-to-fact" conditional. But the point is that in any such process of experimental verification

The simple laws thus obtained may then mutually correct each other, and by this process of mutual correction the complex concrete situation may be reconstructed. The discrepancies, that is, of the ideal case, described by a simple law, a *special* formula, from the *real* case, adequately described by the complex law which is derivable from the special laws by the "compositive" method (for example, the general gas law: $pV = RT$), can be treated as "systematic" errors. A systematic error is an error arising from the intentional neglect of a determinative factor whose mode of operation is known; the partial effect due to that neglected factor may then be calculated, and thus the error due to its neglect is eliminable. In dynamics, each special law of force is, potentially, an instrument for the elimination of systematic errors; for in dynamics the determinative factors whose influence upon the magnitude and direction of motions is "systematically" neglected, are special forces. We have shown how, for example, the law of friction is used to correct the law of motion on an inclined frictionless plane (cf p. 83).

The resolute method has significant epistemological implications with regard to the problem of the physical meaning of the concepts and laws of physics. Philosophical criticism of the "abstractions" of physics is often rooted in an oversight of the *instrumental* character of those abstractions. It is but ignorance of the resolute-compositive method of mathematical physics that can lend any plausibility to the Humean principle "no idea without corresponding impression," if this principle is taken as a criterion of what constitutes a meaningful physical hypothesis. If Hume's principle is to have any relevance to scientific practice, it must not be interpreted to refer to scientific hypotheses in general, but only to those ultimate deductive consequences of scientific hypotheses in terms of which the latter are verified. The "ideas" in terms of which the hypotheses themselves are formulated need not "correspond" to observables, only the ideas in terms of which the deductive consequences are formulated have to satisfy this requirement. If a set of hypotheses yields consequences that describe the "appearances," then the concepts in terms of which each constituent hypothesis is formulated are valid physical concepts, even though nothing in our perceptual ex-

systematic errors have to be considered, and that without such corrections a law like the law of Boyle-Marotte would be more often refuted than confirmed by the experimental data. Thus, a pressure calculated on the basis of Boyle's law may not accord with measured pressure values on account of a rise of temperature. One may then use Charles' law to calculate the pressure difference caused by this rise of temperature. The pressure calculated in terms of both Boyle's and Charles' law will more closely approximate to the "real" pressure.

perience may correspond to them. In mechanics, such constituent hypotheses are differential equations of the second order, involving the concept "instantaneous acceleration." Surely, no "impression" corresponds to these instantaneous accelerations, much less to their vector components. The deductive consequences of these differential laws are integrated equations, expressing the position of a system as a function of the time. It is these integrated equations that may be said to be descriptive of the observable phenomena, inasmuch as changes of relative position are observable. The differential laws, however, are not "descriptive" laws. They are, to use Natorp's phrase, "Konstruktionsstuecke," and it would be overlooking their *instrumental* status to postulate observable referents for the concepts in terms of which they are formulated. What dynamics intends to *describe* (if this fashionable, yet misleading word is to be used) are changes of relative position. The reason for introducing the concept of acceleration is, that accelerations functionally depend on relative positions in a way which does not change with the time (cf. Part two, II, 3), such that in terms of differential equations of the second order, changes of relative position are predictable. The concept of acceleration thus functions as an *auxiliary* concept, a "middle term," that need not itself have a perceptual referent. If the physicist assumes, for example, a centripetal acceleration directed from a revolving planet towards the sun and inversely proportional to the square of the mean distance of the planet from the sun, he can predict any position of the planet relatively to the sun, as a function of the time. What "impression" is there to correspond to such a centripetal acceleration?

We saw that the resolute method involves what might be called a principle of the continuity of laws: the simpler laws must be derivable as special cases from the more general laws by keeping some of the parameters involved in the latter constant or setting them equal to zero. Now, the method of mechanics to predict *finite* changes of relative position through differential laws that involve the notion of *infinitesimal* changes of place corresponding to infinitesimal increments of time, also presupposes what one might call an *ontological* principle of continuity. Even though it became known, through Weierstrass, that continuity is not a *sufficient* condition for differentiability, it remains the case that it is a *necessary* condition for the latter property: only continuous functions are differentiable. Hence, if the fundamental concept of the differential calculus, the concept of the derivative, is to be *applicable* to the motions of mass points, these motions must be assumed to be con-

tinuous. Boltzmann, in his *Vorlesungen ueber die Principe der Mechanik*, formulates this principle of continuity as follows:

Jedem materiellen Punkte, der zu einer gewissen Zeit gewisse Coordinaten hatte, entspricht zu einer unendlich wenig verschiedenen Zeit ein und nur ein materieller Punkt mit je unendlich wenig verschiedenen Coordinaten, welcher derselbe materielle Punkt heisst, d.h. die Coordinaten jedes materiellen Punktes sind continuierliche Funktionen der Zeit, $x = \varphi(t)$, $y = \psi(t)$, $z = \chi(t)$. (2)

This principle is, in Kantian language, a "constitutive condition" of mechanics, in that it defines (in part) the concept of a particle moving in accordance with differential laws. It is *a priori* in so far as it is logically presupposed by the applicability of the differential calculus to mechanics. Such a constitutive principle, which first *defines* the subject-matter of dynamics by rendering certain mathematical concepts applicable, is what Reichenbach, in *Relativitaetstheorie und Erkenntnis a priori*, calls a "Zuordnungsprinzip." With respect to such "principles of coordination" he says: "... Indem sie die Zuordnung bestimmen, werden durch sie erst die Einzelelemente der Wirklichkeit definiert, und in diesem Sinne sind sie *konstitutiv* fuer den wirklichen Gegenstand."³ Thus the rules of vector analysis would be constitutive conditions of mechanics in that they implicitly define the class of vectors, and thus prescribe necessary conditions to be satisfied by the vector quantities of mechanics. Analogously, the principle of continuity, as formulated above, prescribes a necessary condition to be satisfied by moving particles. Once the concept of a "moving particle" will thus have been defined, within the system of classical mechanics, it will be logically impossible and, *a fortiori*, empirically impossible, that the motions of particles should be discontinuous. An entity that moves discontinuously could not be called a "particle" in the sense in which a particle is implicitly defined by the principle of continuity; just as forces to which the principle of the independence of vectors does not apply could not be called "forces" in the sense in which forces are defined by the vector equation $F = m r$ (cf Part two, I, p. 54).

The principle of the continuity of motion is but a special case of the more general principle of the continuity of physical functions (for mathematical physics, motion is but a special kind of physical function, viz., position as a function of time). We saw that, after a dyadic functional relation has been abstracted from a complex pattern of functionally related variables, and a discrete set of pairs of coordinates has been obtained by measurement, the predictive act of generalisation takes the

form of drawing a simple continuous curve through the points that represent the experimental data. Such intra- and extrapolations involve the assumption that a functional relation which has been verified to hold for a finite set of values of correlated variables, also holds for those values that have not yet been measured and many of which never will be measured. This assumption, inherent in the graphical idealisation of distributions of experimental values, may be referred to as a *principle of uniformity*.

In order to render the statement of this principle as definite as possible, exclusive reference will be made to predictions issuing from experimental measurement of properties and mathematical interpretation of the experimental results. To fix our ideas, we may translate the algebraic problem of finding an analytic function that fits all the experimentally obtained sets of values of selected parameters into the graphical problem of finding a surface that contains all the points representing ordered sets of values of the selected parameters. To simplify matters, let us consider the special case where the space in which points are plotted is 2-dimensional, algebraically speaking, we confine our attention to equations involving only two variables. Now, if the principle of uniformity is taken to assert that a function may be found which "describes" the experimental data that are graphically represented by points in a plane, it is both ambiguous and vague. The ambiguity pertains to the meaning of "description" (a word hardly less abused than the word "explanation," for which it was originally intended as a clearer substitute); what is left vague is just what properties the function is to have.

Let us first point out wherein the ambiguity of the word "description" resides. In Reichenbach's terminology, we must distinguish between *reproductive* and *inductive* description. If the equation by which the physicist seeks to "explain" a set of measured values served only the purpose of reproducing in a concise and simple way a set of experimental results, the way Mill conceived the universal judgment to amount merely to a shorthand summary of singular judgments, then the equation sought for would be descriptive in the sense of "reproductive" description. The only criterion which the curve selected out of the infinity of curves that "fit" the data would have to satisfy would be the *esthetic* or *economic* criterion of simplicity. All that could be meant by saying that the simplest curve is the "true" explanation of the experimental data would be, that it is the *preferred* explanation. But, obviously, when the physicist sets up an equation which conforms to the experimental data, he is not engaged in this merely reproductive kind of

description: to talk a familiar philosophical jargon, his equations have a transcendental reference to the unobserved; the act of explanation is *predictive*. In virtue of the predictive character of such explanations, the choice between alternative functions is not purely subjective, but objectively determined: if the simplest function leads to successful predictions, there is a perfectly innocent good old sense in which it may be said that simplicity is not just a property of our ways of describing Nature, but that Nature herself is simple; or, at least, as Kant remarked somewhere in the *Critique of Judgment*, Nature does us the favor to be such that we can get along with simple descriptions. To avoid misunderstandings, it would be advisable to reserve the word "description" for reproductive description, and to speak of "explanation," wherever extrapolation to the unobserved, or prediction, is concerned. It is certainly a very unusual sense of the word "description" in which the scientist is said to describe what he has not yet observed (and perhaps never will observe, although whatever predictions he makes must in principle be capable of verification).

The essence of the principle of uniformity, in the formulation we are considering,* is the assertion that, given a segment of the entire series of ordered sets of values of certain parameters, it is possible to find an analytic function representing the pervasive structure of the entire series, which fits not only the observed segment of the series, but also the unobserved segments. In the case of *mathematical* infinite series, we first construct a function and can then, by substitution of integers for *n*, *produce* members of the series which exemplify the function; there can be no doubt that all the members of the series fit the constructed function, no more than there can be any doubt that a geometrical figure possesses the properties which it is *defined* to have. In the case of *empirical* series, however, it is the members that are given, and the "general term" or structure of the series has to be inductively derived. That the unobserved segments of an empirical series, such as a series of pressure-volume values, exhibit the same structure as the observed segments, is a merely probable assumption; for, as presumably the most stubborn apriorist would admit, the results of further measurements do not depend on our preconception as to which the pervading function is.

Now, if the principle of uniformity were to postulate that the simplest curve which fits the observed points also fits *all* the unobserved points, it would, first place, be unverifiable, since it is, at least practically, impossible to prolong our empirical series *ad infinitum*: but what is

*cf Reichenbach's article "Die Kausalbehauptung und die Moeglichkeit ihrer empirischen Nachpruefung," in *Erkenntnis*, vol. 3.

worse, many, perhaps most, of the established laws of physics would not satisfy this principle. For most physical laws are true only *within limits*, i.e., for finite ranges of the functionally related variables. Boyle's law, e.g., ceases to be valid for pressures above a determinate upper limit, and Hooke's law is valid only within the elastic limits of the medium which is subjected to stress and strain. On the other hand, if we amend this difficulty by definitely restricting the universal quantifier in the phrase "all the unobserved points," we are in danger to reduce our principle to the tautology: the simplest curve which fits the observed points also fits all the unobserved points, except those which it does not fit. It is thus best to leave the import of "all" indeterminate and to assert that the principle of uniformity is verifiable in the sense in which any synthetic universal proposition is verifiable.

Let us, now, turn to the question of falsifiability. Since it is of the nature of regulative principles that they are not directly refutable in terms of definitely circumscribed empirical evidence, it may be anticipated that the principle of uniformity is not definitely falsifiable. We already alluded to a certain vagueness inherent in the assertion of the possibility of inductive (predictive) description in terms of analytic functions. The vagueness, we said, pertains to the properties which such an explanatory function is to have. Now, as Russell has emphasized (see his essay on "The Notion of Cause," in *Mysticism and Logic*), there is nothing peculiarly astonishing about the possibility of inductive description in terms of *some* analytic function. What is astonishing is that it should be the *simplest* functions that lead to successful predictions. If, now, the methodological principle of uniformity asserts the possibility of inductive description in terms of simple functions, it would seem that it would be definitely refuted if, in a fairly large number of cases of inductive description, it is not the simple functions that lead to successful predictions. Yet, leaving it undecided whether the concept of simplicity is ultimately capable of precise definition at all, we may venture the following assertion: the simplicity of a curve is altogether relative to the nature of the distribution of the points that are to be connected. If, for example, the law to be inductively derived is the law of freely falling bodies, the points that represent simultaneous values of distance and time, very nearly arrange themselves in a parabola. In an intuitive sense of "simplicity," a straight line is simpler than a parabola, and a linear function is simpler than a quadratic function. Accordingly, $f(t^2)$, which is the function ultimately chosen by Galileo after other attempts at inductive description had failed, could not be said to be the simplest function in any *absolute* sense, but only *relatively*

to the data at hand. In particular, which is the simplest function relatively to the data, depends on the density, so to speak, of the data. If, for example, in Galileo's experiment the data are a few widely separated points, not too close to the origin of the coordinate system, the simplest curve relatively to those scarce data will be a straight line. Will the principle under discussion be refuted if subsequent measurements show that the distance fallen is not a linear function of the time? No, for one would say that the data were not sufficiently dense. But that the simplest function is the "true" function, provided the data are sufficiently dense, appears to be a very weak, or almost truistic, assertion, in the light of the following consideration: the denser the set of data, the more nearly the postulate that the true function be the simplest function becomes equivalent to the postulate that the true function be continuous. For continuity of functions signifies, loosely speaking, that to very small increments of the argument there correspond very small increments of the function. Consequently, if the curve joining the points that are very close to each other is not simple, in the sense of making erratic "turns" between closely adjacent points, it is not likely to be continuous.

One way of saving the principle of uniformity, then, is to increase the density of the experimental data, so that, if the chosen function is at all continuous, it must be the simplest function. But the principle of uniformity, in the formulation considered, does not contain any statement as to the number of parameters which the desired function is to be a function of. It is a standard method of physics to explain a discrepancy between predictions made on the basis of a function of a certain form and actual observations, not by changing the form of the function, but by adding causally relevant parameters. By this fruitful method, the principle of uniformity can always be saved by maintaining that not all the relevant parameters have been taken into account. Instead of replacing the simple function $f(p_1, p_2 \dots p_n)$ by a more complicated function $g(p_1, p_2 \dots p_n)$, one retains the *form* of the function, but adds parameters; the function then becomes: $f(p_1, p_2 \dots p_n, p_{n+1} \dots p_{n+r})$, where $p_{n+1} \dots p_{n+r}$ are additional parameters, previously neglected. In graphical terms, this modification expresses itself as an increase in the dimensionality of the space in which points are plotted, while the kind of curve, surface or hypersurface, as the case may be, remains the same. Thus a straight line might be replaced by a plane, if a parameter is added to the argument of a linear function, or a parabola by a paraboloidal surface, if a parameter is added to the argument of a quadratic function.

A classical illustration of such a "saving" of the principle of uniformity by increasing the dimensionality (number of parameters) of a simple analytic function, is the reduction of Boyle's law to a special case of the general gas law $pV = RT$. The protasis clause of Boyle's law contains the condition that the temperature is constant. Finding that the equation $V = c/p$ (c being a constant depending on the mass of the gas) does not lead to successful predictions, the physicist does not reject the *form* of the function $f(V, p)$; instead he suspects that the reason why the points do not arrange themselves along a branch of a rectangular hyperbola is the presence of temperature fluctuations. The plausible thing to do, then, is to retain the form of the function f , but to add the temperature as a parameter; the function thus becomes $f(V, p, T)$. This law in turn applies only for a determinate range of the variables p, V, T : for low temperatures the isothermals of a gas cease to be smooth hyperbolas, and exhibit characteristic discontinuities. Does the physicist conclude that he must try out a different form of the function $f(p, V, T)$, or in fact must choose a *discontinuous* function to represent the experimental data, thus renouncing his faith in the principle that the unobserved is predictable by means of continuous functions? No, he explains the anomaly by the fact that another condition, entering into the antecedent of the gas law, is not satisfied: the gas exhibiting the anomalous behavior, graphically evident from the queer forms of the isothermals below a certain temperature, does not have the character of an *ideal gas*. To reinstitute the desired *adequatio intellectus et rei*, the average molecular velocity (to be evaluated in terms of pressure and density) is added to the relevant parameters, and thus the general gas law is transformed into Van der Waal's equation of state for *real gases*: $(p + a/v^2)(V - b) = RT$ (where a and b are constants, differing in their values from gas to gas)

As was mentioned already, the idealizing representation of experimental data by *continuous* functions is necessary if it is desired to apply the differential calculus to physical phenomena. Continuity is a necessary condition for differentiability, hence, if the physicist wants the derivative of the function under investigation to exist, he must assume the latter to be continuous.* What is the physical significance of such

*Boltzmann (op cit, vol I, p 12) points out that the differentiability of the "empirically given" functions which mechanics deals with, is a contingent fact and cannot be deduced from the differentiability of analytic functions. Now, it must certainly be admitted that the applicability of differentiable functions to empirical phenomena cannot be demonstrated by a priori reasoning. It is logically possible that the discontinuity of certain motions should call for difference equations in terms of which the position of a moving subject, such as an electron, could not be predicted for *any* time, but only for the limits of finite time intervals. On the other hand, it is too crudely empiricistic a notion to

derivatives? They are physical constants. If the equation of the curve obtained by inter- and extrapolation is of the first degree, like the linear equation expressing Hooke's law, then already the first derivative of the function with respect to the argument is a constant; in the case of Hooke's law, the first derivative is a modulus of elasticity. If the equation established by inductive generalisation is of the second degree, two successive acts of differentiation will be required to obtain a physical constant. For example, if the function whose continuity is assumed for the sake of differentiation, is the position of a projectile (moving in a vacuum) as a function of the time, the law has the form of an equation of a parabola ($s = s_0 + v_0 t + \frac{1}{2}gt^2$) and the second order derivative which represents a physical constant is the acceleration of gravity. The assertion that such physical constants exist is equivalent to the assertion that the linear or quadratic equations whose differentiation yields those constants, are, physically interpreted, true. Whether Hooke's law is true, and whether constants of elasticity exist, whether Galileo's law of freely falling bodies is true, and whether uniform central accelerations exist, whether the law of thermal expansion is true, and whether coefficients of thermal expansion exist: these are but different ways of asking the same question. The postulate that physical functions are continuous is thus presupposed by the postulate that physical constants, mathematically expressed as derivatives, exist; practically, the former postulate may even be said to be equivalent to the latter postulate, for those rare functions that are continuous but nowhere differentiable have hardly any physical application.

Would not these correlative categories of continuity and constancy be condemned by Hume's sensationalist principle as unreal "fictions"? What "impression" is there in our sense experience to correspond to the mathematical concept of continuity?

. . . All we really know about a particle may be said to be a discrete set of points representing its successive positions in a given reference-frame. Our assumption is that the particle has traversed a *continuous* path which is to be represented as the limit of the vector sum of elementary displacements.⁴

This consideration may, of course, be generalised such as to apply to physical functions other than spatial position as well. Our senses, even when assisted by the most sensitive measuring instruments, cannot

think that functions are "empirically given" at all, at least they are not empirically given *qua* differentiable. If we *find* empirical functions to be differentiable, it is because we have *made* them continuous. Differentiability, that is, is a "constitutive condition" that enters into the very application of the mathematical function concept to empirical phenomena

reveal to us continuous or infinitely divisible quantities. All measurement presupposes discrete units to start with. What then is the significance of the postulate of continuity for *experimental* physics? It functions as a "regulative idea": it postulates unlimited approximation to the "true" values of the measured quantities. On account of the limited sensitivity of measuring instruments, a given experimental (measured) value always corresponds to an infinity of theoretical (calculated) values.* There is always an infinite number of theoretically incompatible predicted values that are *experimentally equivalent*, since the difference between any two of these predicted values is smaller than the least count (smallest division, or "step") of the appropriate measuring instrument. One of the aims of refining techniques of measurement is to diminish the difference between experimentally equivalent theoretical values ever more; but since the least count of a measuring instrument cannot be made infinitesimal ("as small as we please"), this difference can never be reduced to zero.

An important consequence of this experimental equivalence of theoretically incompatible predictions is, that the continuity of physical functions, in the mathematical sense of "continuity,"† could not possibly be *discovered*, and hence must be regarded as a methodological *postulate*. A function $f(x)$ is said to be continuous in the neighborhood of x_0 , if by making the difference between (x_0) and (x_1) as small as we please, we can make the difference between $f(x_0)$ and $f(x_1)$ as small as we please. The possibility of making the difference between values of the argument within a given interval as small as we please, implies that the argument may assume *any* value within that interval; and *mutatis mutandis*, the same holds for the function. But since physical quantities, on account of the limited sensitivity of measuring instruments, do not possess this property of infinite divisibility, it is clear that the continuity of physical quantities is not detectable by measurement and that even the utmost refinement of measuring techniques could not render it an *empirically decidable* question whether physical quantities are "ultimately" continuous or discontinuous.

If we cannot detect the effect of adding a magnitude ξ † to x when $x = x_0$, then x can only have one value within the range $x_0 - \xi$ to

*cf. Duhem, *La Théorie Physique* (Paris 1914), p. 229.

† $f(x)$ is said to be continuous in the neighborhood of x_0 , if there is a δ , such that for every positive $h < \delta$, $|f(x_0 + h) - f(x_0)| < \epsilon$, where ϵ is any positive real number, however small. Or if $f(x)$ is continuous at x_0 , then $\lim_{h \rightarrow 0} f(x_0 + h) = f(x_0)$.

$h \rightarrow 0$

‡ It is to be understood that ξ is equal to or smaller than the least count of the measuring instrument used.

$x_0 + \xi$. There can be no physical meaning whatever in asserting that x has more than one value in this range; the statements that $x = x_1$ and $x = x_2$, where x_1 and x_2 are both within the range, can only assert precisely the same experimental facts; experimentally there can be no distinction whatever.*

The statement that physical quantities are continuous is, then, indeed, *a priori* in the sense of being irrefutable by experiment; an allegedly discovered discontinuity could always be explained by the finiteness of the least count of the measuring instrument. If such a principle of continuity is *a priori*, one will suspect that it is not "about reality." One may, indeed, interpret it as a methodological postulate, a statement about, or rather an imperative addressed to, inquiry: it postulates unlimited refinement of methods of measurement. It may be expressed as the postulate to make the interval of possible experimental error ever smaller by increasing the sensitivity of our measuring instruments. According to it, it is possible to approximate indefinitely to the elimination of this error interval*, even though it is impossible ever to attain to this elimination.

We have discussed a principle of the continuity of laws, operative in the systematization of empirical knowledge, as well as a principle of the continuity of physical functions and a principle of uniformity, both of them operative in mathematical interpretations of experimental data. These principles may be properly called "methodological postulates" or "rules of procedure"; it being implied by this terminology that they are not directly statements about the *subject-matter* of science, but rather imperatives addressed to scientific *procedure*. If the term "rule of procedure" is applied to such methodological postulates, it is confusing to apply it also to laws that are not *directly* descriptive and hence not *directly* refutable by negative instances. Laws referring to ideal contrary-to-fact situations are neither analytic nor irrefutable by experiment, although their difference from inductive generalisations as they are current in classificatory sciences (botany, zoology) deserves attention. If by a "rule of procedure," in Felix Kaufmann's sense, one understands a definition, with respect to a given context of scientific inquiry, of "warranted assertibility" or "correct scientific decision," it

*It should be noted that the above statement holds only for measurements of macroscopic (observable) quantities, not for the determination of values of microscopic parameters. For, according to Heisenberg's "principle of indeterminacy," there exists a definite limit for the attainable degree of precision of measurements upon "conjugate" parameters like the position and momentum of electrons, that is, even though the error in measurements of one of the conjugate parameters may be indefinitely diminished, the product of the errors in measurements of both has a definite minimum.

is surely using terms inconsistently to call such non-descriptive laws analytical rules of procedure Kaufmann acknowledges that the distinction between "empirical" and "theoretical" laws was suggested to him by Dewey's distinction between "generic" and "universal" propositions (cf. Part one, II). Indeed, it suffers from an analogous defect: a functional property is treated as an inherent property. The paradox that all numerical (functional) laws should *be* rules of procedure resolves itself if, in the language here adopted, we say that all numerical laws *may function analytically*. Whenever, for example, experimental conditions are re-examined because the predictions made with the help of a numerical law fail to agree with the experimental data, an empirical law operates as a "procedural rule" (cf. Part one, IV)

Functions must not be converted into "entities," constituting authentic "realms." Thus the fact that empirical laws may have a procedural function as means of interpreting and "correcting" experimental data in no way compels one to say that they are really "rules of procedure" and "conventions." If a given synthetic universal proposition (an empirical law) is to be *uniquely* decided (i.e., verified or falsified) by experiment, other synthetic propositions must, in that context of experimental testing, function analytically. As the *ceteris paribus* proviso, which (explicitly or implicitly) enters into the antecedent of any empirical law, signifies, before an empirical law can be pronounced as refuted by experiment, systematic errors must be eliminated. For example, the apparent failure, in a given experiment, of Hooke's law, may be explained in terms of a thermal expansion of the test wire, due to an unforeseen rise of temperature in the laboratory. In correcting the measured values of the strain, one uses the law of thermal expansion as an instrument for eliminating a systematic error. But it would be absurd to conclude that the law of thermal expansion *is* a "rule of procedure."

As long as laws are available by whose help systematic errors are eliminable, the apparent failure of a law may always be explained away by saying that the *ceteris paribus* condition was not fulfilled. In general, if the accidental errors have been taken into account, by substituting for the variables of a numerical law the arithmetic means of the discrepant measured values, and the law still fails to be verified, one will conjecture the presence of some cause of systematic error. The word "conjecture" is essential: if the existence of systematic error were *apodictically* inferred from the failure of a law to be confirmed by experimental data, then, obviously, no law could ever be refuted by experiment. However, the existence of systematic error is a *hypothesis* which will be verified only if the "disturbance" can itself be brought under a law. To correct a

law by elimination of systematic errors then simply means to supplement it by another law which renders the discrepancy from the predicted results itself predictable. The extension of the principle of the conservation of mechanical energy by means of the discovery of the mechanical equivalent of heat is a classical illustration of this approximation to "reality" by a "superposition" of abstract laws. Unless the relation between frictional dissipation of mechanical energy and heat could itself be formulated as a law (work done against friction is proportional to heat developed, the factor of proportionality being Joule's mechanical equivalent of heat), it would be idle to insist that the conservation principle does not really fail, there being present some cause of systematic error. As has been shown in this concluding section, "approximation to reality" by the superposition of laws takes in mathematical physics the form of generalisation by correction of the discrepancies that are due to the idealizing neglect of relevant conditions.

NOTES

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1. C. I. Lewis, *Mind and the World Order*, New York, Scribner, 1929, p. 293.
2. *ibid*, p. 303.
3. L. S. Stebbing, *The A Priori*, Proceedings of the Aristotelian Society, Supplementary Volume XII, pp. 188-89.

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SECTION I

1. *op cit*, p. 224
2. *ibid*, p. 224
3. *cf op cit*, pp 295-297.
4. *ibid*, p 294
5. *loc cit*, p 433.

SECTION II

1. *cf Die Kausalitaet in der gegenwaertigen Physik*, in *Gesammelte Aufsaeitze*, p. 57.
2. Rudolf Carnap, *Logische Syntax der Sprache*, p 139.
3. Dewey, *Logic, The Theory of Inquiry*, New York, 1938, p. 257.
4. *ibid.*, p 274

SECTION III

1. John Stuart Mill, *System of Logic*, book I, ch 8, section 5.
2. Painlevé, *Les axiomes de la Mécanique* (Paris 1922), p. 54.
3. Victor Lenzen, *Reason in Science*, University of California Publications in Philosophy, vol 21, p 88

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1. Poincaré, *La Valeur de la Science*, Nouvelle édition des oeuvres philosophiques d'Henri Poincaré, par Gustave Le Bon, p. 260
2. Julius Weinberg, *An Examination of Logical Positivism*, New York, London, 1936, p. 143.

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1. Poincaré, *La Science et l'Hypothèse*, Nouvelle édition des oeuvres philosophiques d'Henri Poincaré, par Gustave Le Bon, p 119.
2. Russell, *The Principles of Mathematics*, 2nd edition, London, 1937, p 464
3. *ibid*, § 456
4. *ibid.*, § 465
5. Broad, *Perception, Physics and Reality*, Cambridge, 1914, p. 313
6. Ernst Mach, *Die Mechanik in ihrer Entwicklung*, historisch und kritisch dargestellt, Leipzig, Brockhaus, 1908, p 257.
7. Poincaré, *La Science et l'Hypothèse*, p. 119.
8. *op cit*, p. 124.
9. Karl Pearson, *The Grammar of Science* (London 1911), p 301.
10. Cournot, *Traité sur l'enchaînement des idées fondamentales dans les sciences et dans l'histoire*, Paris, 1911, p 199.
11. Poincaré, *op. cit.*, p. 133.

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1. Kant, *Vorlesungen zur Logik*, §3, Anmerkung 1.

- 2 Planck, *Das Prinzip der Erhaltung der Energie*, Leipzig, 1887, p. 96.
3. Painlevé, *op cit*, p. 9.
4. Planck, *op cit*, p. 107.
- 5 Newton, *Philosophiae Naturalis Principia Mathematica*, scholium ad definitionem IV.
- 6 Enriques, *Probleme der Wissenschaft*, German translation by Kurt Grelling, vol. II, p. 272
- 7 Reichenbach, *Philosophie der Raum-Zeit Lehre*, Berlin, 1928, p. 38.
- 8 *ibid*, p. 44
- 9 C D Broad, "Kant's theory of mathematical and philosophical reasoning," *Proceedings of the Aristotelian Society*, 1941-42, p. 12.
- 10 Russell, *The Foundations of Geometry*, Cambridge, 1897, p. 77.
- 11 Poincaré, "Des Fondements de la Géometrie," *Revue de Métaphysique et de la Morale*, 1899, p. 267.

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1. Felix Kaufmann, *Methodology of the Social Sciences*, New York, 1944, p. 87.
2. Boltzmann, *Vorlesungen ueber die Principe der Mechanik*, Leipzig, 1897-1904, p. 9.
3. *op cit*, p. 50.
4. Lindsay and Margenau, *Foundations of Physics*, New York, 1936, p. 83.
5. Norman Campbell, *Physics, the Elements*, Cambridge, 1926, p. 539.

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